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SOME CHEMICAL ASPECTS OF THE ORIGIN OF PETROLEUM[1]

By Professor S. C. LIND

DIRECTOR OF THE SCHOOL OF CHEMISTRY, UNIVERSITY OF MINNESOTA

THE continuing abundance of the natural supply of liquid hydrocarbons in the form of petroleum is the modern version of the "widow's cruse" magnified to a twentieth-century scale. Little less miraculous than its abundance is the remarkable chemical complexity of the liquid mixture found in nature. No one knows the total list of hydrocarbons that compose petroleum. An old and, as it now appears, unjust charge against the petroleum industry was that it did not make sufficient efforts to learn the complete catalogue of the constituents of petroleum, multiple and variable though they were known to be.

For the past several years the American Petroleum Institute has concerned itself with the identification of the chemical constituents of certain commercial fractions of petroleum. The work has been ably conducted by Dr. E. W. Washburn, of the Bureau of Standards, and his associates. The researches which are still in progress continue to add to the evidence of the astounding complexity of even a single commercial fraction of petroleum. Several hundred fractions were obtained before the identification of a single chemical individual became possible. One of their recent achievements[2] is the proof that further fractionation is still proceeding when difference of boiling point becomes so slight (less than 0.01° C.) that it fails as a guide, while difference in index of refraction still detects progressing fractionation.

The synthesis of liquid hydrocarbons from a single gaseous member by certain methods which will be discussed later has also produced very complex mixtures. This was to be expected from the theory of the mechanism developed before the actual complexity

[1] Address of the vice-president and retiring chairman of Section C—Chemistry, American Association for the Advancement of Science, Cleveland, December 30, 1930.

[2] *Bull.* Amer. Petroleum Inst., Vol. XI, No. 53, p. 7, Sept. 12, 1930.

was demonstrated. The methods also suggested for the first time direct means of building up heavier hydrocarbon molecules from lighter ones, in contrast with the generally known process of breaking down heavy to light ones by thermal means, as illustrated by cracking.

The great chemical complexity of the liquid hydrocarbons existing in nature raises certain very interesting chemical questions as to the origin of this degree of complexity without involving directly the problem of ultimate origin. The solution offered, or the new evidence in favor of it, may, however, throw some indirect light on the larger problem. But it is really the origin of the complexity rather than the initial source of natural hydrocarbons toward which the present discussion is directed.

Evidently, one must admit either a definite chemical reactivity among hydrocarbons enabling them to spread out from a certain center consisting of one or a small number of members in both directions, higher and lower, to give the observed complexity, or else we must admit the complexity as existent from the primary origin itself.

On *a priori* grounds, the latter alternative has always appeared to the writer highly improbable, that such a variety of molecular species should all originate from a single source by any natural process in a single step either chemical or biological. If, then, a process is conceived by which a whole series of hydrocarbons can be produced from one original member by a succession of steps under such conditions as may exist in nature, such a process must appear to have the higher degree of probability.

We encounter, however, the difficulty that saturated hydrocarbons have not generally been credited with great chemical reactivity toward each other, especially not in the process of building higher from lower ones. If the organic chemist desires to add together two hydrocarbon molecules, he resorts to roundabout and most unnatural processes, such as substituting a chlorine atom in each, then removing the two halogens simultaneously by the vigorous action of a strongly basic metal like sodium, with the object of adding the two free hydrocarbon radicals together.

Since it would be impossible or highly improbable for this kind of synthesis to occur in nature, and if no simpler one could be conceived, an organic origin of petroleum from animal or vegetable sources would be favored, since an initially high molecular weight followed by thermal degradation would be required.

Let us examine the general thermodynamic relations in reactions between saturated hydrocarbons. Generally, they involve but small free chemical energy. For this reason, there is no driving force to cause interaction, and under ordinary conditions they *are* quite inactive toward each other. On the other hand, since there is no driving force in any direction, there is no large opposing force to be overcome; in other words, the heats of reaction are low. Furthermore, if suitable conditions are found to produce reaction, and since there are no large directing forces, one may expect reaction to take place in any direction and to proceed by successive steps in all directions, hence leading to great complexity of product, without having to assume such variety in the original source.

A thermodynamic treatment of possible reactions between hydrocarbons has been recently given by Professor H. A. Wilson[3] of the Rice Institute. Between successive members of the paraffine series in the region of temperatures of several hundred degrees and pressures from a few to several hundred atmospheres he assumes equilibria of the type:

$$2\ C_nH_{2n+2} \rightleftarrows C_{n+1}H_{2(n+1)+2} + C_{n-1}H_{2(n-1)+2}.$$

Thus there exists an equilibrium, dependent on pressure and temperature, between any member of the paraffine series and the members next above and next below in the series in point of number of carbon atoms. Since the relation is perfectly general, it will extend in both directions: to methane, ethane, at the lower, gaseous, end of the series, and to solid members at the upper end. The light gases will be either trapped, and thus furnish the gas pressure associated with petroleum, or, if liberated, will allow the dynamic equilibrium to cause a chemical drift of the hydrocarbons continuously from higher toward lower members.

Professor Wilson has pointed out that so far as his calculations are concerned, the equilibria might be purely physical ones among preexisting members of the series, but it seems quite justifiable to make the step, as he does in his later paper, "Theory of Cracking,"[4] of applying the same considerations to equilibria arrived at by chemical action. To the writer, it appears entirely logical to extend the reasoning to the processes of petroleum generation, regarding cracking and petroleum formation as entirely similar processes from the chemical standpoint, after making due allowances for such differences of physical conditions as would be necessary.

With this kind of mechanism, it would be possible then under influence of such temperatures and pressures as may quite reasonably exist in the earth's upper crust, to start with any member of the paraffine series and arrive at a liquid mixture like petroleum.

The olefine series has been given similar thermo-

[3] Proc. Roy. Soc. 116A, 501 (1927); 120A, 247 (1928).
[4] Proc. Roy. Soc. 124A, 16 (1930).

dynamic treatment by Professor Wilson.[5] It offers no new difficulties. Equilibria similar to those among the paraffines may be set up. Transition from paraffines to olefines could be effected by the elimination of methane. The treatment promised by Professor Wilson for benzene and the other ring series has not yet been given. It is an interesting question whether a transition from chain to ring members could be accomplished by inorganic processes in nature or whether the ring members originated in life processes in the primary source. While there is some evidence of the generation of benzene derivatives from paraffines by some of the vigorous agents to be next discussed, it is by no means conclusive as yet.

Besides the reactivity of hydrocarbons under the influence of heat and pressure just discussed, other more vigorous agents such as electrical discharge,[6] alpha radiation,[7] and ultra-violet light[8] have been found effective in causing them to interact. While all of the characteristics of these various types of agents are not as yet understood, they have one property in common which is rather surprising. In spite of the large quantum amounts of energy applied, the hydrocarbons are not generally broken down, but exhibit the striking property of condensation to form liquids or solids, with only so much elimination of lower gaseous members as is necessary to avoid chemical supersaturation.

Even when spectrographic evidence indicates an intermediate dissociation of high degree, as in the recent work of Harkins and Gans,[9] subsequent action leads to additional products in solid and extremely inert states.

The preciseness of the results obtained in the action of alpha rays on gaseous hydrocarbons has permitted the development of a theory of the reaction mechanism. Whether it be direct action between ions and molecules or interaction of free radicals does not concern the present discussion vitally. The predominant result is condensation of lower to higher members on up into the region of liquids and solids.[10] The theory gained from the alpha-ray studies also predicts a great variety of products both in the paraffine and olefine series. Starting from a single member either high or low should lead to all other members above and below, distributed according to some form of probability curve with its peak at the member having double the number of carbon atoms of that in the original molecule. The theory is confirmed among gaseous members, but owing to the scant quantity of liquid obtained under alpha radiation, fractionation of the liquid products has not been attempted. However, the same theory has been found applicable to liquids obtained by electrical discharge in hydrocarbons.[11] The quantities of liquid thus obtained have been sufficient to permit of some fractionation. Great complexity is revealed, as expected, and highest abundance for the molecular species with the number of carbon atoms double that of the original species; for example, octane from butane.

The application of some of these processes to the origin of petroleum may appear remote. Indeed it is so, except as the general principle may apply that when a certain type of reactions or a set of products is demonstrated to be possible through the employment of some special agent like ultra-violet light or alpha radiation, the probability becomes greater that some conditions of temperature, pressure or catalysis exist which render the same reactions possible with a lower quantum expense of energy.

Although electrical discharge and ultra-violet radiation are abundant in the earth's atmosphere, they are unknown in the crust and hence can play no rôle in synthesis of petroleum. But, not so with alpha radiation, which, due to the universal radioactivity of the crust, is everywhere present, though in very low intensity. In their original consideration of the chemical behavior of hydrocarbons under alpha rays, Lind and Bardwell pointed out[12] that feeble intensity of radiation might be so compensated by prolonged action through geological periods of time as to suggest a theory of the origin of petroleum from gaseous hydrocarbons under the influence of alpha radiation, provided two apparent obstacles could be overcome. First, are there conditions in the crust under which an appreciable fraction of the alpha radiation could be absorbed by hydrocarbons? Since the "porosity" of gas and oil sands reaches the value of 20 per cent., it seemed probable that the fraction of alpha radiation effectively absorbed in the petroleum structure might approach that degree of efficiency. The second obstacle appears more formidable. The action of alpha rays on all members of the paraffine and olefine series was found to result in the liberation of much hydrogen; whereas in all natural gases occurring in the United States, hydrogen is notably absent.

[5] *Loc. cit.*

[6] Lind and Glockler, *J. A. C. S.*, 50, 1767 (1928); 51, 2811, 3655 (1929); 52, 4450 (1930).

[7] Lind and Bardwell, *ibid.*, 48, 1556; 2335 (1926).

[8] *Stan. Tolloczko, Przem. Chem.*, 11, 245 (1927); Taylor and Hill, *J. A. C. S.*, 51, 2922 (1929); *Viktor Kemula, Roczn. Chem.*, 10, 273 (1930).

[9] *J. A. C. S.*, 52, 2578; 5165 (1930).

[10] W. T. Richards, Proc. Camb. Phil. Soc., 23, 516 (1927).

[11] Lind and Glockler, *J. A. C. S.*, 52, 4450 (1930).

[12] *Loc. cit.*

In the purely thermal processes of Professor Wilson, liberation of hydrogen is not assumed. This alone would appear to make his mechanism the more probable. But neglecting this hydrogen difficulty, if we calculate the amount of petroleum in the earth's crust that would correspond to the present total of helium content of the atmosphere, on the basis that each atom originated in the crust as an alpha particle, a large total is arrived at. This calculation has been made by Farr and Rogers[13] on the basis of 100 per cent. efficiency in the utilization of the alpha-ray energy in producing petroleum and assuming the same yield per ion pair as found by Lind and Bardwell experimentally. The estimated total of two billion tons for the Petrolia Field of Texas is so huge that even after making large allowances for overestimation of energy utilization, yield, etc., the balance could still exceed the actual production.[14] Corrections in the opposite direction, such as possible loss of helium from the atmosphere leaving the present total content too low, and helium in natural gases still remaining in the earth would raise the total possible. The calculations of Farr and Rogers also have the advantage of being independent of any time factor. It may be mentioned incidentally that some recent analyses of natural gases in New Zealand by the same authors report as much as 4 to 20 per cent. of hydrogen in ten out of eighty-two samples, though the helium content in none of them exceeded 0.02 per cent.

To sum up, it may be said that we now know processes either thermal or ionic by which progression both up and down the hydrocarbon series is effected, starting from any member in the series. This leads directly to the complexity found in natural petroleums, as is also found in the electrically synthesized ones. Consequently, the starting material, whether of vegetable, animal, or mineral source, does not need to be a complex mixture, but may be a single chemical species, from which a high degree of complexity is obtained by steps which appear simple and natural when the chemical and thermodynamic properties of hydrocarbons are taken into account. The simplicity of such a mechanism may lend indirect support to the old idea of an inorganic origin from one or a few hydrocarbon gases such as might be produced by the action of water on metallic carbides in the earth's interior. On the other hand, it does not preclude animal or vegetable origin, but strongly suggests that the primary material, whether gaseous, liquid or solid, is later subjected to thermal (or ionic) agents (or both) which produce the complexity found in nature.

[13] *Nature*, 121, 938 (1928); M. N. Rogers, *New Zealand Journ. Sci. and Technol.*, 11, 389 (1930).

[14] It is also to be remembered that only about 20 per cent. of the oil contained in a structure is actually recovered.

SOME RECENT ASPECTS OF NEMATOLOGY[1]

By Dr. N. A. COBB

U. S. DEPARTMENT OF AGRICULTURE, WASHINGTON, D. C.

ZOOLOGICAL text-books give nemas inadequate treatment—treatment altogether disproportionate to their scientific and practical importance; the space devoted to nemas is insufficient, while many of the statements are antiquated and erroneous.

In judging this defect about 250 zoological and biological text-books printed in English were examined, including text-books proper and books often recommended to students for collateral reading.

A review of these books arouses the suspicion that the text-book treatment given the nemas instead of improving has retrograded. Certain text-books of fifty years ago, now no longer used, give this phylum better treatment than is often the case with texts of to-day.

To cover recent practice it was decided to examine carefully only latest editions. These were grouped, 32 of zoology, and 28 of biology. As a definite basis of comparison seemed necessary, it was decided to compare the nemas with the echinoderms and with the protozoa—a selection determined in part by the following consideration. A cursory examination showed that very much more space is given the echinoderms than the nemas. Since both groups are regarded as phyla and since the two groups present something near the same degree of complexity of organization and since both have long been known to science, it was thought they would furnish material for an illuminating comparison. Reasons for comparison with the protozoa will be presented later.

The percentages of text-book space given the phyla were compared, as well as the number and quality of the illustrations. The percentage of space occupied in each case was taken as a basis of comparison in order that the size of the page and of the type might safely be disregarded.

The 32 text-books of zoology devoted, on the aver-

[1] Extract from the 1929 presidential address before the American Society of Parasitologists, American Association for the Advancement of Science, Des Moines, Iowa.

age, over three times as much space to echinoderms as to nemas (3.12 per cent. to 1.02 per cent.). There was a still greater disparity in the matter of illustrations—fully three times as many devoted to the echinoderms—but in addition the figures illustrating the echinoderms often were works of art—large, attractive and showing much detail, while those devoted to the nemas were sometimes the very reverse.

A moment ago the text-book treatment of the nemas was characterized as not only inadequate but antiquated. It is sincerely to be wished that criticism might end there; unfortunately it can not. In the space devoted to nemas an almost unbelievable number of misstatements occur in this series of 30 odd zoological texts in use at the present time in colleges and universities. The errors, both of commission and omission, are almost incredible, sometimes an average of 5 to 10 serious errors per page. In one or two cases there are no misstatements of fact, but in these cases, unfortunately, the nemas are almost wholly ignored.

At what may perhaps be some risk, *e.g.*, the risk of being thought a scold, I venture to point out some of these errors—about ten errors of a serious character, a dozen or more of a somewhat less serious character and errors of a minor character.

"J'ACCUSE"

(1) I accuse the authors, almost without exception, of failing to state the long-established fact that nemas moult. That all nemas moult has never been proved absolutely, any more than it has for insects, but the nemas known to moult are sufficiently numerous and varied to justify the assumption that all nemas moult, and that this is a most significant and fundamental feature of their development, which, as its details become fully known, will, in due time, doubtless aid in determining the relationship of nemas to other phyla.

(2) I accuse them, almost without exception, of failing to note the very highly significant fact that nemas do not present ciliated tissue.

(3) I accuse them of failing even to mention the spinneret, one of the most peculiar of organs, as characteristic in its way as the spinnerets of spiders. In fact a parallel omission would be a chapter on spiders ignoring their spinnerets. When this exceedingly characteristic nemic organ is absent or obscure, it is usually for the same reason as in certain groups of arachnids—degeneration due to parasitism.

(4) I accuse them of failure to note the absence of typical striated muscular tissue.

(5) I accuse them, with very few exceptions indeed, of either stating or creating the impression that Ascaris is a typical nema. This is very far indeed from the truth; it is hardly more true than that certain wingless and legless parasitic bugs are typical of all insects, or that the turtles are typical of all reptiles, or the monotremes of mammals. We know at the present time as many free-living nemic genera as parasitic, the number of known free-living species being the greater and very much the more varied. As might be expected under such circumstances, the parasitic forms show a marked degeneration of many features characteristic of the free-living forms in their own phylum—the very forms from which, according to accepted doctrine, they must have evolved. Now Ascaris partakes in this degeneration. There are very many important characters belonging to, and widely distributed in, the free-living forms of this phylum which are so nearly absent in Ascaris that they are no longer noticeable. The domination of Ascaris in this section of zoological text-books is so complete as to be something of a catastrophe in the teaching of this phylum.

(6) I accuse them—in fact they accuse themselves, for "qui s'excuse s'accuse"—I accuse them of so little knowledge of the nemic phylum, with comparatively few exceptions, as to include, nearly always apologetically, in the space devoted to nemas, organisms that do not belong there, and on the other hand excluding organisms that do belong there. There is no sound morphological reason for classing the Gordiaceae, the Acanthocephala or the Chaetognaths in the nemic phylum; yet this is still a common procedure. It is equally erroneous to exclude from the nemas the groups Chaetosomatidae and Desmoscolecidae. It would be just as reasonable to exclude the turtles from the Reptilia as to place Desmoscolex outside the nemas. Desmoscolex (and Chaetosoma) are typical nemas in a comparatively strict sense of the word; their internal organization is strictly nemic, but is masked by a modification of the exterior that has, unfortunately, led to error on the part of even noted zoologists. But these errors are of long ago and have since been again and again shown to be such.

(7) I accuse them of leaving unexplained the simple and fundamental fact that the cuticle of nemas, being non-compressible along the lateral lines, constitutes an exoskeleton, acting on which, two antagonistic systems of muscles, one ventrad and the other dorsad, effect all ordinary body movements in the dorso-ventral plane. Nemas do not, can not, move as do eels, by bending laterally.

(8) I accuse the great majority of them of omitting even to mention free-living nemas; so far as many of the text-books are concerned, the student might remain practically unaware that free-living nemas exist, and yet they constitute quite half of the known number of forms, and most undoubtedly, in many respects,

the more important half, for it is certainly correct that the true character of nemas will be fully disclosed only by a study of their more highly developed free-living forms.

(9) I accuse the majority of them not only of giving to students the impression that nemas are parasites mainly, if not entirely, but accuse them, even in presenting *this* point of view, of omitting to mention the very important fact that plants are parasitized by nemas. To show how important an omission this is, one has only to call attention to the well-known fact that the gall nema, a nemic parasite of the roots of over 700 species of plants, including most of our crop plants, is one of the worst pests known to agriculture, the annual losses from which to the world are measured in hundreds of millions of dollars. And yet this is only one of many species that infest plants. True, it is the worst of its class, but many of the others are very serious—so much so sometimes as to have ruffled international relationships and to have been the subject of laws regulating international and interstate commerce.

(10) I accuse them of misleading statements concerning the excretory system of nemas. It is regularly stated that the two lateral "lines" (meaning lateral chords) *contain* the excretory vessels, the inference being that the chords are excretory. Now it is true that in *Ascaris megalocephala* the excretory vessels lie in the lateral chords, but this is not true even of all the species that have been classed as Ascaris, for in some of them one of the lateral chords has nothing to do with the excretory vessels. In a large number of other parasitic forms the statement would not be true, while for almost all free-living forms the statement is utterly untrue. Probably the excretory vessels are not physiologically connected with the chords in any case whatever—even among parasites. The reason that the excretory system is sometimes imbedded in, or attached to, the lateral chords, particularly in some of the larger forms, is a mechanical one. This is the region in which these long tubular organs can be stowed with least inconvenience, and this is the main, and in fact probably the whole, reason for their occasional association with the lateral chords. These facts were published about forty years ago.

(11) There is no mention made of longitudinal chords or fields other than the two lateral ones, in the face of published observations to the contrary that are half a century old, observations that have been corroborated over and over again and have long been common knowledge among nematologists, and in one very large group have even long been used as characters for the separation of genera. These chords are a basic feature of the nemic anatomy—wellsprings of the cuticle.

These are serious errors. Could any one be seriously blamed for asking whether teaching them does not come too near being an imposition?

ADDITIONAL THOUGH SOMEWHAT LESS SERIOUS ERRORS

Point one. Refers to the text-book statement that hermaphroditism is rare in nemas. Hermaphroditism is not uncommon among free-living nemas—relatively as wide-spread as in insects—and it is becoming known among the parasitic species. Numerous and widely varying genera of free-living nemas present species, sometimes a considerable fraction of the species of the genus, in which the males are rare or non-existent. Under such circumstances, the females develop their own sperms and these sperms are efficient, at least to the extent of inciting development of the eggs.

Point two. Either the statement is made, or it is assumed, that nemas have no locomotor organs.[2] You have already been shown the nature of the locomotor organs of numerous nemas. There no longer exists the slightest doubt that well-developed locomotor organs are present on hundreds of species of nemas belonging to a variety of genera.

Point three. Influenced no doubt by a limited knowledge of the organization of Ascaris, the statement is made that the only sense organs of nemas are papillae on the lips. Here and there, it is admitted, eyespots exist in a few forms. Now the formerly so-called "lateral organs" have been taken for chemical sense organs for a quarter of a century. They are universal in free-living nemas, and it is now becoming very evident that they are universal in parasitic nemas. It is more than twenty-five years since these organs were designated sense organs, and this idea is now so thoroughly established as to need no further comment. Phototropes of very considerable complexity, probably in some cases entitled to be regarded as organs of vision, exist in not a few of the free-living nemas—a score or more of widely varying genera. As you have been shown, these may be so complicated as to possess image-producing lenses, pigmented receptors and special nerves connecting them with the central nervous system. In addition there are tactile organs in various parts of the body, supplied with special nerves and existing in both parasitic and free-living forms, and beyond doubt universal. There are special ganglia connected with the sexual functions and these ganglia are associated with special organs, long interpreted as sense organs. There is no lack of sense organs in the nemas. How could there be?

[2] This part of the address was preceded by a lantern-slide review of the morphology and physiology of nemas.

Point four. Mainly a point of omission. The student is given very little idea of the complexity of the nemic organization. By direct statement and by omission the text encourages him to consider the nemic organism as of a simple character. Now the structure of nemas is so complicated that it is in reality one of the marvels of living organization that so many different systems of organs can be packed into a slender microscopic speck only a fraction of a millimeter long, as some are. It will be readily admitted that the late Jacques Loeb was a penetrating observer. He repeatedly said to me, "It is amazing—the complexity of the nematodes. . . . The variety of specific organization in so small a space is marvelous."

Point five. The statement is made that the life history of nemas is usually very complicated. It has become the fashion to describe briefly a few nemas that cause disease in human beings. Trichinella is often selected as an example, and, as its life history appears somewhat complicated, the assumption is made that this is typical of nemas. As a matter of fact, the life history of the vast majority of nemas is about as simple as it could be for organisms of their degree of complexity, no more complex than the life history of rotifers, or that of many animals of other groups of similar complexity. This error undoubtedly is a part of the misinformation connected with the assumption that Trichinella and other parasitic nemas are typical of the nemic phylum.

The dissections of Ascaris made in most zoological laboratories are very limited in their extent, and apparently are usually carried out by those knowing so little of the structure of nemas as to perpetuate the errors that the structure of nemas is simple and the life history complex.

Point six. An important error of omission is failure to recognize the historical significance of nemas and to use the facts of history to implant in the minds of students the historic and scientific importance of the phylum they are studying at the moment. History tells us that in the early eighteen seventies it was in a nema that the male and female animal gametes were first seen to approach each other and "coalesce" (observations of Bütschli) to form the single "pronucleus" from which alone a new individual can arise, speaking broadly. It was in the eggs of nemas that it was first shown that the "chromatin" of the two gametes after thus coming together divided in such a way that chromatin from both gametes (both parents) is distributed to each cell during segmentation, thus pointing out *for the first time* the physical basis of heredity in animals as conceived to-day (discovery of Van Beneden). Following these statements by calling attention to the classical researches of Boveri connected with the eggs of Ascaris and other nemas (the early segregation of the gonadic elements—thus disclosing the continuity of the germ-plasm, etc.; etc.) is sufficient to show the extraordinarily important rôle nemas have played in the development of the science of heredity. Most important and fundamental biological discoveries were made through the instrumentality of nemas.

Point seven. It is said that the specific and generic differences among nemas are slight, in other words that those who have spent years studying the nemas are prone to make much of slight differences. As a matter of fact there is no essential difference—no difference worth discussion in this connection—between the principles guiding nematologists in ranging the nemas into species, genera, families, etc., and the principles that guide naturalists in other phyla. Nemic species are as different from each other as lions and tigers, their genera as different as cats and dogs, and so on up the taxonomic scale.

Point eight. Why continue the use of that antiquated word "worm," with all its looseness of meaning, and by its very use leading the student, perhaps unconsciously, into the assumption that things called worms have a scientific resemblance to each other—that tapeworms and roundworms and flatworms, all of them worms, have some sort of organic resemblance, justifying some sort of assemblage?

Why call any of them worms at all? We are well rid of the old subkingdom Vermes; why retain worms?

Why call nemas roundworms? They are no rounder than what are very often alluded to as worms belonging to other phyla.

Leading students even remotely to associate nemas with trematoids and cestoids may lead them to think that the structure, life history, etc., of nemas are similar to those of these other groups, when as a matter of fact there is very little real resemblance; the differences are very, very great. No doubt the statement made in more than one of the texts that the life history of nemas is usually complicated comes from this very association of nemas with other more purely parasitic groups, under the misleading denomination worms, whose life history *is* entitled to be called complicated, in that it involves regularly in all species the passage of a parasitic form through two or more conditions, forms or hosts.

Point nine. Contrary to well-established chemical knowledge it is common to state that the cuticle, egg shell, etc., of nemas is chitinous—composed of chitin. While there is a superficial resemblance between the cuticle of nemas and that of other phyla, chemists have long since established the fact that the substance mainly composing the cuticle of nemas is not chitin, and that its properties are very different from those of chitin. Among other things it quickly disintegrates

in water when once it is out from under the influence of the living nema itself. Hence the unfortunate fact that we have no fossil nemas to speak of, while fossil insects are known, sometimes in considerable detail, and from ancient strata, owing to the relative insolubility of their exoskeleton.

Point ten. Why give book room to such statements as that nemas live mainly on the juices of living hosts, when as a matter of fact nemas have learned to ingest and digest food of almost inconceivable variety? Within reasonable limits it is hardly possible to make the statement unduly strong.

Point eleven. Why harbor the thought, let alone permit its proclamation in our classrooms, that parasitic nemas show little degeneration in comparison with free-living nemas, when as a matter of fact their degeneration in this respect is practically on a par with the degeneration of parasitic forms in other phyla?

Point twelve. Authors state that nemas are entirely devoid of segmentation, in face of the fact that for a decade or more it has been established beyond peradventure that very many of them, probably the majority of the free-living forms, bear appendages that must be denominated segmented. This matter is incapable of full discussion here, but it is, to say the least, incautious to deny nemas all trace of segmentation.

Point thirteen. In these texts all the free-living forms are still placed in one or only a few families, *e.g.*, Anguillulidae, when as a matter of fact it has long been common knowledge among nematologists that they belong to a wider range of families than do the parasitic nemas.

Most of these additional thirteen errors are also serious ones.

MINOR ERRORS

One text at least continues the mistake of regarding the esophageal swellings as stomachs, or organs for trituration, and even on occasion of calling them "gizzards."

At least one text places the central nervous system at the anterior end, instead of around the esophagus.

The longitudinal chords are called "thickenings of the epidermis" at the same time that they are said to *include* the excretory system, whose embryonic origin is entirely different from that of the epidermis.

Not infrequently the texts treat the nemas as constituting a group of lower order, instead of, as they do, constituting one of the most outstanding phyla of which we have knowledge.

I have come across other misstatements which, though they are not common, are worthy of mention. Nemas are said to be cylindroid and to taper at the two extremities. Among the free-living forms, which, as before remarked, constitute at least half the present known forms, this description of Ascaris applies but poorly. While it is true that they often do taper more or less toward the extremities, not infrequently this is not at all a marked feature, especially in front, and one which in a general description would be ignored. Furthermore, there are large numbers of nemas to which the term cylindroid is totally inapplicable; some are spherical or nearly so, many are much wider toward the extremities than they are in the middle. No doubt those who derive their idea of the form and motion of nemas from Ascaris alone think that all nemas have a serpentine motion. Their movements are *always draconic,* never serpentine. A large number of nemas, hundreds of species, numerous genera, creep after the manner of the caterpillar popularly known as the inchworm or measuring worm, as shown in some of the slides exhibited.

I note a good many misapplications of generic and specific names, but we know so little about the details of this phylum as yet that it is premature to attempt any final or even fairly satisfactory philosophical classification. The classifications necessarily proposed must be looked upon as matters of more or less ephemeral convenience, and usually not as adequate expressions of zoological philosophy. Probably the total number of species now described does not much exceed 5,000, belonging to from 900 to 1,000 genera, distributed about equally among the free-living forms and the parasitic forms. When we consider that those who have given the closest attention to the matter believe the species of nemas existing must be numbered in at least hundreds of thousands, and when, in addition to this, we consider that the great majority —fully nine tenths at least—of the forms that have been seen, studied and named are inadequately known, it becomes evident how futile it is, at the present time, to make strenuous attempts to institute a philosophical classification. Inasmuch as nematologists themselves have come to no very satisfactory conclusion in regard to the more comprehensive taxonomic groups, it is quite forgivable to writers of zoological text-books that errors of this sort occur.

I am not unaware of the difficulties in teaching nematology as it should be taught, nor inexperienced in the matter, having as a teacher actually used nemas in courses in school and university. The difficulties are mainly connected with their small size and the fact that their organs, highly varied though they are in function and form, are reduced to extremely small size and packed into extremely small space. Their study and demonstration therefore require skilful use of the microscope. At one time this would have been

a serious matter. Microscopes of the quality and number necessary for the purpose would have been expensive and difficult to procure. This difficulty has been largely decreased, and we now find microscopes used with more skill than formerly in zoological laboratories, especially in conjunction with protozoology and cytology.

It is for this latter reason that a comparison is now drawn between the treatment given in these same text-books of zoology to nemas and to protozoa. Not because the two groups are morphologically comparable; the comparison is much less apt than the comparison between nemas and echinoderms. It is made for the purpose of showing, among other things, that it is impossible for teachers of zoology longer to plead that it is the small size of nemas and the necessity of using microscopes skilfully that have brought about the condition criticized. If these difficulties can be overcome in connection with the protozoa, there would seem to be no reason why they can not be overcome in the case of nemas. The comparison will show—particularly through the nature of the forms selected by authors to illustrate respectively the nemas and the protozoa—that such an excuse will no longer hold.

As might be expected, the average space given the protozoa is more than five times as great as that given the nemas, and the illustrations outdo both in relative number and quality those devoted to nemas, that is to say there are more than five times as many illustrations given under the head of protozoa as under the head of nemas, and they are better.

A careful examination of the illustrations shows that the microscopy necessary for the production of these particular protozoan illustrations is practically of the same nature as that required for a study of the nemas. Hence if we assume that along with the zoological texts, corresponding laboratory work is done, and done satisfactorily, we must assume that in protozoological laboratories mieroscopes are used with that degree of skill and painstaking care necessary in connection with nemas.

The twenty-eight text-books on biology gave figures very closely comparable with those obtained from text-books on zoology.

The figures and facts presented indicate an opinion on the part of those who prepare text-books of zoology and of those who teach zoology that there are stronger reasons for acquainting students of zoology with echinoderms than with nemas. Let us compare these two phyla (1) with respect to their economic importance, (2) their historical importance, (3) their importance as furnishing suitable material for teaching purposes.

Economic importance. We have already seen, in connection with the lantern slides shown, something of the great importance of nemas as causing diseases of man and his domesticated animals and plants. It is quite impossible to go into detail here, but a few items will be mentioned that show the enormous importance of this phylum in this respect.

For example, I am authoritatively informed that of the approximately twenty-one and one half million dollars the Rockefeller Foundation has spent on public health activities, excluding buildings, equipment and endowment, over one fourth has been spent on what is known as hookworm control. Many government agencies in many lands have contributed cooperatively large additional amounts to the same end at the same time. I leave you to calculate the probable losses to mankind through this single nemic disease, to cause such a huge, world-wide expenditure in an attempt to ameliorate it. In doing so it is well to remember that this is only one of over a hundred distinct species of nemas known to infect the human body.

My colleague, Dr. M. C. Hall, estimates the livestock losses in the United States through the attacks of nemas to be not less than one hundred million dollars per annum.

You have already heard of the huge crop losses to agriculture due to the attacks of the gall nema.

The oft-repeated statement is amply justified, that nemas are responsible for annual losses aggregating very many millions of dollars, and for death, suffering and inefficiency on a large scale among human beings and their domesticated animals and plants.

To offset all this I am glad to say I know of no echinoderm causing a serious disease of human beings, or of a domesticated animal or plant. The economic losses due to echinoderms are confined to the depredations of predatory forms, and even here the list is not very impressive. The marine shellfish industry, or at any rate the oyster industry, suffers locally, at times severely, from the attacks of starfish, but if the entire marine shellfish industry were thus wiped out, the loss would not be equivalent to any one of many single items connected with nemic diseases.

Let us now consider the balance on the other side. It is not so widely known as it should be that there are beneficial nemas, that is, nemas beneficial to mankind because they are active enemies of other organisms which on their part are injurious to mankind. We are only at the beginning of these important lines of research, and yet investigations have already shown that the prevalence of a number of very injurious insects is largely dependent upon whether or not they are parasitized by certain nemas. The nemas are such an important factor in the prevalence of some of these insects as to appear second only to the reproductive powers of the insects themselves, and in some

cases it is becoming rather difficult to see how human beings could live in comfort in certain regions now thickly populated were it not for the beneficent effect of certain nemas.

Benefits from echinoderms are practically confined to one item, the value of trepang, or bêche-de-mer, a food—more or less of a luxury—used in the East. The total value of the trepang industry, which is said to employ hundreds of vessels, is a figure I have been unable to obtain with exactness, so I resort to liberal estimates. If we place the entire fleet engaged in the industry at a thousand vessels, which I believe too high, and the average annual value of the catch of each schooner at $10,000 per annum, which is probably in excess of reality, the total annual valuation of the catch would be $10,000,000. The trepang actually fished from the Great Barrier reef, by far the largest fishing ground, is given by the Queensland government as about 33,000 pounds sterling per annum, say $155,000, and from this I am inclined to think the estimates given above too large. The trepang fished on the Pacific Coast of the United States, 1927, is given by the Bureau of Fisheries as 5,355 pounds, valued at $268.

Historical and scientific importance. In comparing the relative historical and scientific importance of nemas and echinoderms, I can hardly do better than refer to that classical work, Wilson's "The Cell in Development and Heredity," a work of broad scope dealing comprehensively with what constituted the main biological work of the late nineteenth and the early twentieth centuries, a work by general consent placed in the very first rank.

If the reader of this work will consult its index to authors, he will find that few if any authors are more frequently and extensively quoted than Van Beneden and Boveri. Remove from the text the passages and illustrations derived from these particular researches of these two authors, and the very heart would be taken out of the work. If now one inquires what were the organisms used by these men in their epoch-making discoveries he will find that they were very largely, in fact in many cases almost exclusively, nemas. It would be difficult to conceive more convincing evidence of the great historical importance of nemas as contributory material for some of the most fundamental biological researches of the last 50 years.

Very many important researches, and many of a basic character, have been carried out with the aid of echinoderms, but the conclusion seems unavoidable that up to the present, in fundamental biological research, they have not been as important as the nemas.

Relative importance of nemas and echinoderms as a source of laboratory material for teaching zoology. Many echinoderms are of large size and are readily collected, preserved and shipped. In their living condition they are interesting and often attractive objects. Where running sea water is available, the study of their gametes, fertilization and early development is fascinating and highly instructive work. A practical disadvantage is that they are marine only.

One can therefore understand why echinoderms have received some of the attention remarked upon in this review of zoological texts.

The nemic laboratory material currently used in our zoological courses suffers severely by comparison. In fact, it is rather difficult to imagine a more uninteresting, not to say disgusting, object to be placed in front of, say, a dainty and refined girl student than *Ascaris,* so stimulative of disagreeable feelings and thoughts, and devoid of a single curious or interesting external feature to attract attention.

Fortunately, however, it is not necessary to introduce nematology to students by way of *Ascaris,* although at present any other course is little heard of in our schools and universities. There is a large assortment of extremely interesting microscopic nemas that can be placed before the student in a living state—nemas both free-living and parasitic—and when this is done with the aid of good microscopy there is in my personal oft-repeated experience never any lack of interest, or even enthusiasm, on the part of students, whether they be new to biology or already considerably advanced.

This successful and attractive way of introducing live nemas to students can be carried out almost anywhere, but necessitates good microscopy and involves certain comparatively simple technique long used in certain laboratories. Through this change in material and methods, the nemas may easily be made so attractive as pedagogically to compare favorably with other organisms.

Whatever the method of comparison adopted, we are unable to come to a conclusion justifying the present relatively small amount of space and time assigned to nemas in zoological texts and courses. The emphasis seems so obviously misplaced as to lead to the following constructive suggestions.

(1) Rectify those egregious errors with regard to nemas. Most of them are no longer excusable.

(2) Cut down the space and time devoted to, say, echinoderms, by 50 per cent. or more and add them to the nemas.

(3) Teach nematology through the instrumentality of living, microscopic, transparent forms, especially the free-living ones, and relegate *Ascaris* to the background so far as morphology is concerned.

These suggestions are brief but comprehensive. Probably the greatest obstacle to their immediate

adoption is the fact that so few trained zoologists know anything worth speaking of concerning nemas. It is suggested that trained zoologists can instruct themselves by a perusal of original nemic literature (not text-books—not encyclopedias) available in most large libraries, and by a few weeks study of living nemas with the aid of high-power immersion lenses. The nemas should be under sufficient pressure to prevent active motion, but not sufficient to altogether prevent them from moving.

THE CHALLENGE OF PLANT VIRUS DIFFERENTIATION AND CLASSIFICATION[1]

By Professor JAMES JOHNSON and Dr. ISME A. HOGGAN

UNIVERSITY OF WISCONSIN AND U. S. DEPARTMENT OF AGRICULTURE

FOR some time there has been no phase of phytopathology in greater need of cooperative thinking and action than that of plant virus differentiation and classification. Much uncertainty and confusion have existed in this field of investigation ever since the first recognition of a virus disease by Adolph Mayer in 1886. Soon after Mayer's work became known, it was claimed by some that his "Mosaikkrankheit" of tobacco included two distinct diseases, one the true infectious mosaic and the other a supposedly unrelated necrotic disease known elsewhere as "Pockenkrankheit." Although much attention was given to the subject, this disagreement has persisted almost to the present time, though it is now quite generally conceded that Mayer was correct in his interpretation that necrosis is one of the symptoms of the ordinary tobacco mosaic virus on tobacco as well as on certain other hosts.

In the meantime, the continued description of virus diseases on different hosts, on the basis of symptoms only, has led to serious confusion even in fundamental research concerning the nature of a virus. It was natural that a school of thought should develop which was inclined to the belief that only one, or at the most only a few, viruses existed in nature, or that a virus was a labile entity capable of adapting itself to various hosts and circumstances. This point of view has only recently been dispelled by those who maintain that many distinct and specific viruses exist in nature and that we have in the viruses a problem of differentiation and classification comparable in complexity if not in extent to that in mycology and bacteriology.

Unfortunately, however, the pendulum is apparently swinging too rapidly in this direction. The tendency to apply new names to a virus disease when only symptom expression is involved, either on an old or on a new host, is leading to new difficulties, the more serious because we are dealing with an unseen entity, the true nature of which may long remain a mystery.

The challenge is clearly before the workers on plant viruses, first, to check themselves and others as far as possible from adding to our present difficulties, and then to clear up as rapidly as may be done the confusion already existing in the literature.

While the reliable methods now available for the differentiation of plant viruses are not applicable in all cases nor entirely satisfactory in others, such methods are yet remarkably useful considering the early stage of development of this subject. New and better methods for the differentiation, determination and description of specific viruses are gradually becoming available, and important advances in this line of technique may be looked for in the future. Many of these methods are already familiar to most of the workers. In connection with a discussion of this kind, it may be well briefly to list the more obvious of these methods and to discuss their possibilities and their limitations.

Four chief types of differential or diagnostic features of plant viruses are recognized at the present time. These are: symptom expression, properties of the virus, modes of transmission and the cytological picture.

SYMPTOM EXPRESSION

Comparative symptoms on a single host species or variety have constituted the main diagnostic character relied on up to the present time in the recognition of specific viruses. The best example of the use of this type of differentiation lies of course in the potato virus group. The limitations of this method, useful as it has been in the past, are obvious to any one who has worked with this group of diseases. The symptoms produced may vary greatly with the variety of potato and its stage of development, and with the source and method of infection as well as with the environment. Consequently, descriptions of symptoms of the different viruses often overlap so extensively as to be quite unreliable even to authori-

[1] Paper read before the Section of Mycology and Plant Pathology of the Fifth International Botanical Congress at Cambridge, England, August 20, 1930.

ties on the subject, who may unconsciously depend rather upon recognition through acquaintance than through description. However, all that is distinctly useful in this method of differentiation should be retained, but there is much in the way of detailed description which could advantageously be dispensed with.

The use of comparative symptoms on a range of different host species or varieties is a diagnostic character which has been less commonly used than the possibilities appear to justify. It is fundamental to the progress of virus investigation to know something of the host range of each virus, and where the host ranges are distinct, or even where the symptoms are distinct on some hosts although not on others, a good basis for differentiation or determination exists. It must be admitted that, in general, the use of the differential host method up to the present time has often served to confuse rather than to simplify our problem, but we believe that, with new facts which have been brought to light, the situation will soon be reversed.

A third group of differential factors which warrant consideration in the present connection is that of environmental influences, such as the effect of temperature on symptom expression. The remarkable influence of temperature on the symptom expression of certain potato viruses is now well known. Temperatures which may mask one virus disease may intensify the symptoms of another, as, for example, in the case of crinkle mosaic and rugose mosaic of the potato, using Schultz's terminology. This characteristic may consequently be of considerable value in the determination of these and other diseases where other more simple or reliable methods may not be at hand. In any case, factors of environment and other related circumstances, such as vigor and stage of development of the host, need to be taken into consideration in any attempted determination or description of a virus disease on the basis of symptom expression.

PROPERTIES OF THE VIRUS

The properties of the virus itself seem to offer the most reliable and satisfactory characteristics for differentiation of certain viruses of the true mosaic type, or of those viruses which are quite readily artificially inoculable from plant extracts, as distinguished from those transmissible only by insects or by grafting, which are commonly of the "yellows" type of virus disease. Remarkable differences exist between these mosaic viruses with respect to the length of time they may survive in plant extract or apart from the living host. It is necessary only to call attention to the tobacco mosaic virus, which may apparently live outside the living host for as long as twenty-five years under certain conditions, in contrast to a certain potato mosaic virus which apparently becomes inactivated in two to four hours. Similarly, tolerances to dilution may vary from 1 to 10 to 1 to 10,000 or more with different viruses, and thermal death-points from 40° C. to 90° C. Corresponding variations in reaction to treatment with chemicals of various sorts are also known to exist, although this field as a means of virus differentiation is as yet relatively unexplored.

Not only are these properties of great value for diagnostic and descriptive purposes, but they may often serve, especially in combination with the use of differential hosts, as a ready means of separating combinations of viruses into their component parts. We have here a beginning of the technique for the isolation of what might be called "pure cultures" of viruses, which, followed up with some modification of Koch's postulates, may eventually place the determination of the mosaic type of virus disease on quite as sound a basis as that now available for diseases of bacterial origin.

MODES OF TRANSMISSION

The third means of differentiation of plant viruses, which merits more consideration than has been given to it from this point of view, is that of differential modes of transmission. We may pass over with bare mention the fact that it is possible in some instances to differentiate viruses by their behavior with respect to the source from which the inoculum is taken, and the method of inoculation used. Tobacco mosaic, for instance, is not recoverable from certain host plants although these may be readily infected with the disease; and Dr. Goss has shown, for example, that the spindle-tuber disease of the potato is transmissible by the cutting knife, whereas the common mosaic viruses of this host are not. Obviously, many viruses may be distinguished on the basis of their differential transmissibility through budding or grafting, insect vectors and virus extracts.

More interesting, however, and possessing greater possibilities for expansion within closely related groups is the isolation and differentiation of plant viruses through their specificity in insect transmission. The development of a method of differentiation on the basis of their insect relationships, particularly with respect to those viruses which are not readily transmissible by artificial means, may eventually serve to complete a satisfactory key for the determination of plant viruses in general. Our knowledge of this field is already sufficient to indicate that we are dealing here with at least three specific conditions affecting the transmissibility of a virus by an insect, namely: (1) The species of insect

involved; (2) the specific virus concerned; and (3) the species of host plant serving as a source of infection. It may also conceivably develop that the species of host plant serving as "suscept" may add to the possibilities of differentiation.

In some cases, at least, a highly specific relationship is known to exist between the virus and its insect vector. Certain diseases of the "yellows" type appear to be transmissible by a single species of leafhopper only, and, so far as is known at present, by no other type of insect; curly top of sugar beet by *Eutettix tenellus;* aster yellows by *Cicadula sexnotata;* and streak disease of maize by *Balclutha mbila.* Although the host ranges of the first two diseases are wide and may overlap to a certain extent, this specific relationship offers a ready means for the isolation and determination of the respective viruses concerned. Again, of the virus diseases affecting the raspberry, for example, the aphid *Aphis rubiphila* is said to spread curl only, and the aphid *Amphorophora rubi* the mosaic diseases only; and other examples may be quoted of different viruses which may affect the same host plant, each dependent upon a different specific insect carrier for its transmission.

It has furthermore been shown that cucumber mosaic is readily transmissible from tobacco by several different species of aphid, while ordinary tobacco mosaic is not so transmissible. Here again is a means of differentiation and a simple method for the separation of the two viruses involved should they occur in combination.

Only a single example may be cited at the present time of the influence of the host species serving as the source of infection on the transmissibility of a virus by an insect. The aphid *Myzus pseudosolani* is apparently unable to transmit the ordinary tobacco mosaic virus from tobacco and certain other solanaceous hosts, yet it will readily transmit this same virus from tomato. Although no adequate explanation of this peculiar host relationship can be offered at the present time, it is evident that here, at least, the species of mosaic host plant may exert a determining influence on the amount of insect transmission of a particular virus. Whether or not this is an entirely exceptional case, however, remains to be determined.

On the other hand, the relationship between insect and virus does not always appear to be so specific. Cucumber mosaic, for example, is said to be transmissible by at least five different species of aphid, as well as by two species of cucumber beetle. Further, the peach aphid has been reported as transmitting a number of different virus diseases, such as spinach blight, various potato mosaics, potato leafroll, lettuce mosaic, sugar-beet mosaic, celery mosaic, bean mosaic, and mosaic of Chinese cabbage, mustard and turnip. Although it has not yet been shown that the various diseases just named are actually due in all cases to different, specific viruses, our knowledge of some of them being practically confined to the symptomatology on a single host, yet several of these are definitely recognized as distinct; and it would consequently appear that the differentiation of viruses by means of certain insect vectors may be somewhat limited in its application.

The Cytological Picture

The fourth and last type of differentiation which we wish to mention is that of the cytological picture in the virus-affected tissues. This method is, of course, often used in the determination of certain animal viruses, although it has not yet been extensively developed for the plant viruses. It has been shown that the so-called "x-bodies," or vacuolate inclusions, are invariably associated with the tobacco mosaic virus regardless of the host on which it exists, provided that mottling or chlorotic symptoms are produced, but that they are not found in the case of the cucumber mosaic virus and certain other viruses on the same hosts even though host mottling may occur. The cytological picture of the potato viruses has hardly been studied sufficiently to warrant any definite conclusions, but we suspect that the details differ here also with certain different viruses. Characteristic cell inclusions are known to be constantly associated also with certain other plant virus diseases, and these may eventually prove to be a valuable diagnostic feature.

The usefulness of the cytological method will, of course, depend upon whether or not any simpler, quicker or more convenient means of differentiation exists where determination is required. The suggestion is merely put forward that cytological technique may eventually prove to be the best method of differentiating two or more specific viruses which are otherwise closely similar.

By the use of the various differential characteristics which we have now discussed, it has already been shown in certain instances that virus diseases of various hosts described in the literature are, or may be, due to one particular specific virus. The cucumber mosaic virus has, for instance, been shown to be the causal agent of mosaic diseases of a number of host species, where this relation was not suspected when the diseases themselves were originally described. There is room for considerably more reduction in synonymy than has so far been achieved. On the other hand, there has been, and there no doubt will be, a growing list of specific plant viruses

adequately described and accepted on both old and new hosts. The first challenge now before us is, however, whether there is any justification for a person to describe and name a virus disease on any host without adequately and thoroughly subjecting the virus concerned to a sufficient number of the differential tests available to ascertain whether or not the virus or the disease in question should be given a new name.

In America this problem of the promiscuous application of new names to virus diseases on the basis of symptom expression only has become so serious that it is generally felt that some concerted action should be taken for the protection of the virus workers themselves, as well as of those of the teachers and students of the future who may be obliged to cope with the subject. A strong feeling existed, therefore, at the last meeting of the American Phytopathological Society that it would be well to have a group of pathologists assigned to consider ways and means of reducing the difficulties before us. The initiative in this direction, to be most effective, should come rather from an international body of pathologists. A closely related phase of the subject of virus differentiation is the standardization of the requisite technique. Manifestly, a uniform procedure should be adopted in the determination of the properties of virus extracts. We are also obliged to recognize that the source of the inoculum itself with respect to the host species or variety as well as to other conditions may have a bearing on the results obtained. Finally, it must be recognized that the host plants to which the inoculum is applied may respond differently according to their age and vigor and to the surrounding environmental conditions. The subject of standardization of technique is one in which a good beginning could be made by the selection of some international group to help lead the way.

We are perhaps not yet sufficiently far advanced to go far into the field of strict classification of the plant viruses. Those of us who have attempted to comprehend the viruses as a group, however, are impressed by the fact that we appear to have several closely related classes or forms which may be compared to species of a single genus, while other groups of viruses are as distinct, certainly, as the most widely separated groups of bacteria. The development of a system of classification for the viruses seems to be almost inevitable in the near future, while this is at the same time a matter in which we can afford to move slowly.

The adoption of a uniform system of nomenclature for the viruses would prove to be highly desirable to the students of the subject. There appears to be no serious obstacle in the way of some satisfactory international agreement on this subject. Several proposals have already been made in the literature, but we wish to point out here that the effort should be fundamentally in the direction of naming the virus rather than the disease which it causes. In practice we may never overcome the synonymy and confusion of the common names of plant diseases, but there is no good reason why a single technical name should not be made to represent a specific disease-producing entity.

We have purposely taken this unusual opportunity to make such an appeal, rather than to present actual details of results and conclusions in this field of investigation. If the challenge of virus differentiation problems is to be met, we are convinced that nothing more helpful could come about than for some international body to come to some agreement on a system for plant virus differentiation, classification and nomenclature, and to use its best influence to secure the universal adoption of such a system or standard as will eventually place the subject of plant viruses in a position commensurate with their importance in the sciences.

SCIENTIFIC EVENTS

VIVISECTION IN ENGLAND

A BILL has been introduced in the House of Commons by Lieutenant-Commander Kenworthy to prevent the application of public moneys to vivisection experiments. The measure is a subsidiary bill promoted by the British Union for Abolition of Vivisection, and was previously before Parliament in 1922 and 1924, according to the London *Times*.

The British Medical Association is opposed to the bill and has addressed a letter to members of Parliament in which it is pointed out that the Act of 1876 lays down that no one but the holder of a license from the Secretary of State is permitted to use animals for experiments; that such work shall only be carried out at registered places; and that the experiments must be performed with a view to the advancement of physiological knowledge or of knowledge which will be useful for saving or prolonging life or alleviating suffering.

The letter of the association continues:

> This work is loosely termed vivisection, but no severe cutting operation is permitted under the Act without the use of an anesthetic of sufficient power to prevent the animal feeling pain. Very many of the so-called ex-

periments permitted under the Act are done for the routine purpose of public health or of medical treatment for the immediate benefit of the community or individual patients. The potency of many remedies in use to-day, notably glandular extract, vaccines sera, and some drugs, such as arsenicals (*e.g.*, salvarsan) can not be determined except by animal experiments. Without being so tested they may be uselessly weak or dangerously strong. Lives depend upon these powerful medicaments being of standard strength.

Should the expenditure of public money on such work as this be prohibited? Pituitary extract is a good example. It is a valuable drug in childbirth, often diminishing pain and danger and obviating the use of instruments, but an overdose might easily kill the patient. Before proper control was introduced, different preparations on the market varied in strength up to as high a ratio as 80 to 1 with results that can be imagined. By means of animal experiments a standard of potency, expressed in definite units, has been secured, and is, in fact, now enforced by law. This is one of the national biological standards for which the Medical Research Council is responsible. Some infectious diseases, *e.g.*, some cases of tuberculosis, can not be diagnosed with certainty except by animal tests, and animals must of necessity be used in the preparation of certain vaccines and sera. It is a public duty that such work as this should be carried out, and in some cases the law requires it to be done. The effective control of therapeutic substances can only be ensured by the state, and therefore by the expenditure of public funds. It is not always realized that the term vivisection covers such work as this, and the British Medical Association is of opinion it is in the interests of the community that Commander Kenworthy's Bill should be opposed.

REORGANIZATION OF THE NATIONAL PARKS ASSOCIATION

AT a special meeting on December 5, 1930, the Board of Trustees of the National Parks Association was reorganized by election of members appointed by twenty-two leading scientific and conservational organizations, and unanimously adopted the following statement offered by Dr. John C. Merriam and seconded by Dr. Wallace W. Atwood, president of the association:

The National Parks Association should be so organized as to speak with the authority of accurate knowledge on problems touching use and future development of National Parks. It should consist of representative individuals and representatives of organizations in a position to see the great problem of the parks from the point of view of physical, emotional, intellectual and spiritual values. It should be a body able to think park problems through, and give accurate and sound expression of judgment on these questions. Among other matters, it should devote itself to:

1. Study of the future function and use of National Parks as a guide in determining how to maintain the proper balance between protection of primitive features in the parks and development of these areas for the purpose of making them accessible to the people.

2. Consideration of future growth of the National Park System on the basis of clear understanding of its use and function. What should be the relation of this system to city parks, state parks, state forests and national forests? What types of areas should be included, and why? What methods should be used in securing new park areas?

On December 24, the following study committees were appointed:

To study future functions and use as stated in the first of the two problems above: Dr. Frederick V. Coville, Mr. Charles W. Eliot, 2d, Dr. Vernon Kellogg, Mr. Duncan McDuffie, Mr. Frederick Law Olmsted, Dr. Victor E. Shelford, Dr. Fred E. Wright and Dr. Wallace W. Atwood, chairman.

To study growth, relationships, types and methods of creation as stated in the second problem above: Mr. Albert W. Atwood, Dr. Theodore S. Palmer, Dr. Henry Baldwin Ward, Mr. David White and Mr. William P. Wharton, chairman.

The new Board of Trustees consists of twenty-two members appointed by prominent associations interested in the attainment of the highest purposes of the National Parks System, and fifteen members at large. Representatives of organizations are:

Otis William Caldwell representing the American Association for the Advancement of Science.

Morse A. Cartwright representing the American Association of Adult Education.

James McKeen Cattell representing the National Academy of Sciences.

Allen Chamberlain representing the Appalachian Mountain Club.

Guy N. Collins representing the American Society of Naturalists.

Frederick V. Coville representing the Botanical Society of America.

William B. Greely representing the Camp Fire Club of America.

George H. Harvey, Jr., representing the Colorado Mountain Club.

Augustus S. Houghton representing the American Game Protective Association.

Vernon Kellogg representing the National Research Council.

George F. Kunz representing the American Scenic and Historic Preservation Society.

Charles Riborg Mann representing the American Council on Education.

Duncan McDuffie representing the Sierra Club.

Frederick Law Olmsted representing the American Society of Landscape Architects.

Theodore S. Palmer representing the American Ornithologists Union.

T. Gilbert Pearson representing the National Association of Audubon Societies.

Arthur Stanley Riggs representing the Archeological Institute of America.

Victor E. Shelford representing the Ecological Society of America.

Mrs. Katharine B. Tippetts representing the General Federation of Women's Clubs.

Henry Baldwin Ward representing the Izaak Walton League of America.

William P. Wharton representing the American Forestry Association.

David White representing the Geological Society of America.

Besides the representatives of other organizations, the reorganized board contains these fifteen trustees at large: Truman Abbe, Albert W. Atwood, Henry W. de Forest, Charles W. Eliot, 2d, Joshua Evans, Jr., Francis M. Goodwin, Caspar W. Hodgson, Walter Bruce Howe, John C. Merriam, John Barton Payne, George E. Scott, Mrs. John Dickinson Sherman, Fred E. Wright and Robert Sterling Yard. Former trustees will be found scattered among both groups, most of them in the latter.

The wholesome relationship of public lands of all uses, of which the National Parks System is one, is the concern of this association, and the preservation of the rare remaining primitive in its safest form of administration is a prime objective.

ROBERT STERLING YARD,
Executive Secretary

THE BOWDOIN INSTITUTE OF NATURAL SCIENCE

BOWDOIN COLLEGE at Brunswick is planning to open on April 7 an institute of natural sciences. The institute will be the fifth in a series begun in 1923, when the subject chosen was "Modern History," and continued at intervals of two years with "Modern Literature," "Art" and "The Social Sciences." The institutes have sought to bring to the student body, the community and the friends of the college an opportunity to hear something of the problems which claim the attention of the leaders in the several fields of activity represented.

Plans for the coming season have been in the hands of a faculty committee under the leadership of Professor Manton Copeland, of the department of biology. Nine speakers will come to the college for the institute, which will occupy about two weeks and will be opened on April 7. Specific fields of science to be represented are astronomy, biology, chemistry, geography, geology, medicine, physics and psychology.

Professor Julian Huxley, English writer and scientist, was the first speaker in the institute group. He spoke at Bowdoin on "Development, Heredity and Evolution."

Dr. Harlow Shapley, director of the Harvard College Observatory, will talk at the April sessions on "The Cosmic Panorama." Professor Kirtley F. Mather, of Harvard, will speak on "Sons of the Earth: The Geologist's View of History."

Professor Dayton C. Miller, of the Case School of Applied Science at Cleveland, will present "Demonstrations on Visible Sound." Professor George H. Parker, Harvard University, will lecture on "The Cost of a Thought." Professor Edwin G. Boring, also of Harvard University, will discuss "The Rise of Scientific Psychology."

Professor Charles H. Herty, of New York, former president of the American Chemical Society, will speak on "Chemistry's Service in the Promotion of Industrial Research in America." Dr. Isaiah Bowman, director of the American Geographical Society, will speak on "The Invitation of the Earth."

Dr. Florence R. Sabin, of the Rockefeller Institute for Medical Research, will have as her topic "Recent Studies of the Chemistry of Bacteria as Applied to Disease." Professor Edwin G. Conklin, of Princeton University, will be the second speaker in the field of biology.

OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

A FULL report of the Cleveland meeting of the American Association for the Advancement of Science and the associated scientific societies will be printed in SCIENCE as soon as the permanent secretary is able to arrange the material. Dr. Franz Boas, professor of anthropology at Columbia University, was elected president, succeeding Dr. Thomas Hunt Morgan, of the California Institute of Technology. Dr. C. F. Roos, assistant professor of mathematics at Cornell University and secretary of Section K, was elected permanent secretary to succeed Dr. Burton E. Livingston, professor of plant physiology at the Johns Hopkins University, who becomes general secretary. Professor Charles A. Shull, University of Chicago, was elected secretary of the council to succeed Professor Sam F. Trelease, of Columbia University. Vice-presidents and chairmen of the sections were elected as follows:

Section A (Mathematics), Dr. Earle R. Hedrick, professor of mathematics, University of California at Los Angeles.

Section B (Physics), Dr. Bergen Davis, professor of physics, Columbia University.

Section C (Chemistry), Dr. C. A. Browne, chief of the Bureau of Chemistry, U. S. Department of Agriculture.

Section D (Astronomy), Dr. J. H. Moore, astronomer, Lick Observatory.

Section E (Geology and Geography), Dr. Douglas Johnson, professor of physiography, Columbia University.

Section F (Zoological Sciences), Dr. Robert Hegner, professor of protozoology, The Johns Hopkins University.

Section G (Botanical Sciences), Dr. Elmer D. Merrill, director, New York Botanical Garden.

Section H (Anthropology), Dr. William K. Gregory, curator of paleontology, American Museum of Natural History, and professor of paleontology, Columbia University.

Section I (Psychology), Dr. Herbert S. Langfeld, professor of psychology, Princeton University.

Section K (Social and Economic Sciences), Dr. Griffith C. Evans, professor of mathematics, Rice Institute.

Section L (Historical and Philological Sciences), Dr. W. B. Munro, professor of history and government, Harvard University, and the California Institute of Technology.

Section M (Engineering), Dr. Dexter S. Kimball, professor of engineering and dean of the College of Engineering, Cornell University.

Section N (Medical Sciences), Professor Howard T. Karsner, professor of pathology, Western Reserve University.

Section O (Agriculture), C. G. Williams, Ohio Agricultural Experiment Station.

Section Q (Education), Professor Ernest Horn, professor of education, State University of Iowa.

Other elections were as follows:

Council members to succeed Dr. L. E. Dickson, University of Chicago, and Dr. David White, of the U. S. Geological Survey, are Dr. F. G. Cottrell, U. S. Department of Agriculture, and Dr. A. F. Woods, U. S. Department of Agriculture.

Members of executive committee to succeed themselves: Dr. J. McKeen Cattell, Garrison, New York, and Professor Henry B. Ward, University of Illinois.

Members of the committee on grants for research to succeed Professor W. Lash Miller, of the University of Toronto, and Professor Oswald Veblen, of Princeton University, are Professor S. C. Lind (chemistry), of the University of Minnesota, and Professor Carl E. Guthe (anthropology), of the University of Michigan.

Member of the finance committee, Herbert A. Gill to succeed himself.

Nomination for board of trustees for Science Service, J. McKeen Cattell to succeed himself.

Secretary of the academy conference, Dr. S. W. Bilsing, of the Texas Agricultural and Mechanical College.

Secretary of the secretaries' conference, Commander N. H. Heck, secretary of Section M.

SCIENTIFIC NOTES AND NEWS

DR. J. PERRIN SMITH, emeritus professor of paleontology at Stanford University, with which he had been connected since 1892, died on January 1. He was sixty-six years old.

IN the British New Year's list of honors Sir Ernest Rutherford, professor of experimental physics and director of the Cavendish Laboratory at the University of Cambridge, has been made a baron; Sir John Rose Bradford, president of the Royal College of Physicians, and Sir Richard Gregory, editor of *Nature,* have been advanced to be baronets; Frank E. Smith, director of scientific research at the admiralty and secretary of the Royal Society, has been knighted.

THE one thousand dollar award of the American Association for the Advancement of Science for an outstanding paper presented at the Cleveland meeting was given to Drs. M. A. Tuve, L. R. Hafstad and O. Dahl, of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington. Their paper entitled "Experiments with High Voltage Tubes" was presented to the American Physical Society.

DR. J. HOWARD BROWN, professor of bacteriology in the School of Medicine of the Johns Hopkins University, was elected president of the Society of American Bacteriologists at the recent meeting at Boston. Dr. L. A. Rogers, of the U. S. Department of Agriculture, and Dr. Norman MacL. Harris, health commissioner of Ottawa, Canada, were elected members of the council. The society will hold its next meeting at the Johns Hopkins Medical School. In 1932 it is planned to meet at the University of Michigan.

DR. ALFRED C. LANE, Pearson professor of geology and mineralogy at Tufts College, was elected president of the Geological Society of America at the forty-third annual meeting held in Toronto.

DR. W. F. G. SWANN, director of the Bartol Research Foundation of the Franklin Institute, was elected president of the American Physical Society at the Cleveland meeting.

DR. FRANCIS CARTER WOOD, of the Crocker Cancer Research Institute of Columbia University, is president-elect of the Radiological Society of North America.

PROFESSOR WILLIAM KING GREGORY was elected president of the Galton Society and Mr. Frederick Osborn secretary-treasurer at the meeting of the society on December 15.

DR. W. D. LAMBERT, of the U. S. Coast and Geodetic Survey, delivered on January 3 the address as retiring president of the Philosophical Society of

Washington. His subject was "The Variation of Latitude."

DR. DAVID WHITE, senior geologist of the U. S. Geological Survey and home secretary of the National Academy of Sciences, was awarded the Penrose Medal at the Toronto meeting of the Society of Economic Geologists.

DR. JULIUS F. RIEDENWALD, of Baltimore, has been awarded the Phi Lambda Kappa medal bestowed annually upon the Jewish physician who has contributed most to the science of medicine.

DR. F. L. DUNLAP, consulting chemist, of Chicago, has been elected a foreign member of the Masaryk Academy of Czechoslovakia.

RAYMOND G. BRESSLER, deputy secretary of agriculture of Pennsylvania, formerly professor at Texas Agricultural and Mechanical College and vice-dean at Pennsylvania State College, has been elected president of Rhode Island State College. He succeeds Dr. Howard Edwards, who died April 9, 1930. Dr. Bressler will take up his work as president on April 1.

DR. F. E. BREITHUT has been made professor of chemistry and head of the department of chemistry of the Brooklyn College of the College of the City of New York.

DR. FORREST D. MCCREA, of Cleveland, has been appointed professor of physiology and pharmacology at Duke University School of Medicine.

DR. O. E. KIESSLING has been appointed chief economist of the mineral statistics division, and Mr. W. W. Adams chief statistician of the newly created demographical division in the U. S. Bureau of Mines. Dr. Kiessling succeeds the late Frank J. Katz, who had served as chief economist of the mineral statistics division economic branch for many years.

MR. SIDNEY SMITH, assistant keeper in the department of Egyptian and Assyrian antiquities at the British Museum, has been appointed keeper of the department in succession to the late Dr. H. R. Hall.

DR. KARL LANDSTEINER, of the Rockefeller Institute for Medical Research, reached New York on January 3 after a visit to Stockholm, where he went to receive the Nobel prize in medicine.

PROFESSOR PARKE HARDY STRUTHERS, head of the Syracuse-Andean Expedition for the study of rare birds, animals and reptiles, with ten collaborators sailed on December 31 on the way to the Sierra Nevada range of the Andes. The party will disembark at Porto Cabello, Venezuela, where they will be joined by natives and proceed inland more than 500 miles to the jungles of Meridia where the expedition will establish its headquarters. Others in the group include Dr. Robert Crockett, bacteriologist; Dr. Ernest Reed, botanist; Dr. Earl T. Apfel, geologist; Professor Sidman Poole, geographer; Wesley Curran, assistant zoologist, and Philip Barnes, photographer. The expedition is to last six months.

Dr. R. A. MILLIKAN, of the California Institute of Technology, delivered a lecture on December 20 before the Royal Canadian Institute on "The Cosmic Rays in Canada."

DR. F. K. RICHTMYER, of Cornell University, gave a popular talk on "X-rays and Their Uses" on November 11 at St. Lawrence University, Canton, New York, under the auspices of the St. Lawrence (Iota) chapter of Sigma Pi Sigma, national honorary physics fraternity.

CAPTAIN DONALD B. MACMILLAN, the explorer, will give three lectures accompanied by motion pictures at the University of California on January 15 and 16. The subjects are "Iceland" and "Northern Lights."

SIR ARTHUR EDDINGTON formally opened on January 6 the twenty-first annual exhibition of the British Physical and Optical Societies at South Kensington. Two lectures with experiments were given on January 7 and 8 by Mr. E. Lancaster-Jones on "Searching for Minerals with Scientific Instruments," and by Sir Gilbert Walker on "Physics of Sport."

AT a meeting of the Galton Society on December 15, Professor G. Elliot Smith, of University College, London, addressed the society on the Peking Man (*Sinanthropus*). Professor Elliot Smith spoke on the same subject before the College of Physicians of Philadelphia on December 11.

THE Second International Congress of Comparative Pathology will meet in Paris, from October 14 to 18. The secretary of the American committee is Dr. George W. McCoy, National Institute of Health, Washington, D. C.

BY the will of the late Rossiter Betts, the residue of his estate amounting to about $1,000,000 is left to Yale University. Legacies of $5,000 each were made to the New York Institute for the Deaf and Dumb, of which the testator was president; the New York Zoological Society, the New York Botanical Garden, the Metropolitan Museum of Art and the American Museum of Natural History. The Hampton Normal and Agricultural Institute at Hampton, Virginia, and the Berry School at Mount Berry, Georgia, receive $2,500 each for scholarships.

MRS. D. A. DUNLAP, as a memorial to her son, has

provided funds for a new astronomical observatory for the University of Toronto to be known as the David Dunlap Observatory. The 24-inch reflecting telescope is already under construction in England, and will be housed in a circular metal building to be erected near the city on a large acreage which will be converted into a park. The observatory will be in charge of the department of astronomy of the University of Toronto, while the faculty of forestry will be in charge of development of the park.

MR. GEORGE EASTMAN, of Rochester, New York, is reported to have given $1,000,000 to the city of Stockholm, Sweden, for a dental dispensary similar to the Rochester Dental Dispensary and to those recently established by Mr. Eastman in London and Rome. The Stockholm clinic will include one feature not provided for in the other foreign cities, a school for dental hygienists similar to that of the Rochester dispensary.

BATTELLE MEMORIAL INSTITUTE at Columbus, Ohio, announces the establishment at the institute of a research project sponsored by the Ohio Steel Foundry Company of Lima and Springfield, Ohio. This will consist of a comprehensive study of steel-foundry practice with a view to developing improvements and economies in practice as well as the betterment of finished products. Dr. C. H. Lorig, a member of the staff and a specialist in foundry practice, will be in immediate charge of this work under the direction of Mr. Clyde E. Williams, assistant director.

FOUR Mississippi State educational institutions were dropped from the eligible list of the American Association of University Professors at the annual meeting at Cleveland. The association voted to condemn wholesale dismissals of faculty members by Governor Theodore G. Bilbo last June and July. The resolution said that "much damage has been done to the cause of education in Mississippi as well as a great injustice to those dismissed or demoted." It charged the 179 dismissals and demotions were made "apparently for political reasons, without due consideration of the welfare of the students affected, and, so far as we are informed, with no notice to those dismissed or demoted." The schools placed on the ineligible list "until such time as the administration of educational affairs in the State of Mississippi has been restored to a status acceptable to this association" are: University of Mississippi, the Agricultural and Mechanical College of Mississippi, the Mississippi State College for Women, and the State Teachers College. This action, effective immediately, means that members can not teach at the institutions and retain their association membership. It was explained that credits of students are not involved, but have been jeopardized by the action of the Southern Association of Colleges and Secondary Schools. The association also adopted a "statement of principle" declaring that "no university professor who receives a fee from any person or association interested in public discussion or in testimony respecting a particular question of public importance should take part in such discussion without making public the fact that he receives such compensation and making public the name of the person or association paying him the said compensation."

THE Czechoslovak Ministry of Education has created a radio section which will have charge of the installation of radios in Czechoslovak schools and the broadcasting of school programs. Special school programs will be broadcast twice daily, according to preliminary plans. Radio sets will be installed in 13,000 primary schools at a cost of approximately 60,000,000 crowns, or about $1,800,000. The larger schools will include the cost of installing radio equipment in their current budgets while arrangements will be made for the smaller schools to receive credit and pay for the equipment over a period of several years.

THE *Journal* of the American Medical Association reports that the erection of the new Institute of Public Health in Rome, in the vicinity of the Policlinico, by the Italian government, toward which the Rockefeller Foundation made a large contribution, was recently begun. The institute will be under the direct control of the public health service and will have functions entirely distinct from those of the university institutes of hygiene. Its chief purpose will be the creation of a school for the education and training of sanitary personnel; it will serve as an aid to the public health service in the country in general, in the provinces and in the communes. The present laboratories of the public health service and of the school for the study of malaria will be reorganized, with new equipment, and will be located in the new institute.

THE Forest Service recently sent congratulations to the Forest School at Eberswalde, Germany, on the school's one-hundredth anniversary. These congratulations are in the form of an illuminated parchment with gold-colored hand-drawn letters and design bearing the message: "The Forest Service of the United States Department of Agriculture congratulates Forstliche Hochschule, Eberswalde, Germany, upon the occasion of its one-hundredth anniversary."

ACTING upon the recommendation of Secretary of the Interior Wilbur, President Hoover by proclamation, dated November 14, added approximately 11,010 acres of land to the Petrified Forest National Monument, Arizona. With this addition the total area of

the monument now is 36,918 acres. The new area contains many features of scientific interest which will be easily accessible to visitors upon the completion of the bridge which the National Park Service plans to build across the Rio Puerco. This bridge is necessary for the convenience of visitors to the Petrified Forest and the new addition contains the most feasible site for its construction. In the past, flood conditions in the river have frequently made impassable the road leading into the forest. The age of the petrified trees, whose fragments cover the ground over an area of over 100 square miles, of which more than half is included in the monument, is estimated at 200,000,000 years. None of the trees is standing. The petrified trunks, more or less fractured, dismembered, and lacking branches, all lie prostrate on or in the ground. Where a trunk or stump is found in an upright position, it is due to tilting of the already petrified log by natural forces. The ancient living forests which supplied these logs did not grow in the location in which they now are found. When the trees fell or were knocked over, they drifted down some prehistoric stream, became waterlogged and sank. Sand and pebbles gathered around and over them and finally thousands of feet of sandstone settled upon them. Changes then took place in the trees, turning them from wooden trunks into a mass of agate and carnelian, still in the shape of the original trees. Then, as the ages went on, there was a slow upheaval of the land, and erosion finally exposed the now thoroughly petrified logs and the innumerable small fragments. In the Petrified Forest National Monument there are three principal forests. Although they are the same geologically, erosion has produced different results in the three areas, and the color and texture of the "wood" also varies considerably.

INVENTIONS made in carrying on research work in the Engineering Experiment Station at the Ohio State University may, at the discretion of the trustees of the institution, be patented either for the benefit of the university or for the benefit of an individual, firm or corporation for which the research work was done, according to a ruling of the Attorney General, Gilbert Bettman, given in response to an inquiry from Dr. George W. Rightmire, president of the university. Persons with whom the university cooperates in experiments have not, by mere force of the relation of the parties, any exclusive rights under the law in inventions made possible as a result of such experiments, the attorney general held, even though such work is financed by funds contributed by them.

DISCUSSION

METEOR BUTTE

THE origin of the geologic formation in Arizona called Meteor Butte (formerly known as Coon Butte), near Sunset Station on the Atchison, Topeka and Santa Fé Railway, has long been considered something of a mystery, although in recent years it has positively been ascribed to the impact of a giant meteor—hence the change of name. The dimensions of the huge basin are: diameter about 4,000 feet, depth 570 feet: a truly remarkable hole in the ground.

In SCIENCE, November 7, 1930, Professor H. L. Fairchild expounds most learnedly his explanation of the origin by collision with a meteor as well as the reason why no vestige of the colliding mass is found either in the butte or anywhere near it.

Inasmuch as no evidence is discoverable of a meteoric body capable of excavating such a large basin, notwithstanding long, competent and diligent examination, and as my poor intelligence sees nothing reported that substantiates in the slightest degree the meteor theory, I venture to disbelieve that theory in toto.

For my part I have always been skeptical in the matter, but I have patiently waited for the drillings and investigations to turn up some credible evidence. No evidence has come that appears to me at all competent.

Mr. D. M. Ballinger, some years ago, caused numerous drillings to be made to considerable depths. He reported the results in an admirable paper read before the National Academy of Sciences in 1909; later printed. This paper he accompanied with a number of very clear diagrams and some excellent photographs of the "butte" taken both inside and outside the so-called crater.

All this drilling and investigating proved one thing *definitely:* that the underlying rock strata are in "continuous and undisturbed position." They also developed the fact that there exists copious ground water. This latter fact is to be specially noted with reference to what follows.

According to Professor Fairchild, whose competency in geology is unquestioned, the topmost continuous stratum is the Kaibab Permian limestone,

250 feet thick, with all the other regular beds below it in position.

Now the key to this problem—the origin of Meteor Butte—seems to me to rest, not in a mythical meteor, but in the presence near the surface of this Kaibab limestone.

It is well known that every rock formation possesses certain peculiarities—certain individual characteristics which we might call "personal" features. One exhibits cross-bedding; another has a tendency to chonchoidal fracture and presents arches and natural bridges; still another yields towers, pinnacles, and so on. This is all too well known to require more than mention.

The peculiarity or "personal" quality of the Kaibab formation is that it has a sponge-like character. On the Kaibab Plateau, whence comes the name of this limestone, there are no brooks or streams flowing on the surface. Instead there are circular drainage basins without apparent inlet or outlet. These basins are of varying size and they are numerous. Their diameter and depth range from an almost imperceptible slope from circumference to center, to several hundred feet in diameter and a hundred feet or more in depth. Some hold water; some do not, most in fact do not.

These sinkholes appear to be the individual feature of the Kaibab limestone. On my first visit there, many years ago, they struck my attention immediately as being something unusual. Dutton was there about the same time and noted the sinkholes as something new.

The explanation seems simple. It is merely a broad downward drainage through porous rock. The Kaibab, of wide extent, is devoid of surface streams, even of the smallest rivulets, yet there is a considerable rainfall, while snow is deep, owing to an altitude of 8,500 feet. The water goes off, of course, but it goes *down* all over the plateau forming these sinkholes.

Doubtless this feature occurs elsewhere but not so prominently as on the Kaibab Plateau. The dissolved rock and other débris is carried down and deposited below where the water reappears as it does in Havasu Canyon and along the breaks of the north wall of the Grand Canyon in the Kaibab Division.

These sinkholes of the Kaibab, some of them at least one fifth as large as Meteor Butte, being in the same limestone that forms the upper structure of Meteor Butte would seem to offer a perfectly reasonable explanation of the origin of Meteor Butte.

That is to say: *Meteor Butte is entirely the work of erosion* and no meteor has had anything to do with its formation. The interior cliffs of the circumference appear, from the photographs, to be cliffs of erosion, for they have every characteristic. The exterior slopes of the circumference appear to be slopes of erosion, for they have every characteristic. The down inside drainage undoubtedly is into the near-by Canyon Diablo.

Where there are local tiltings and dislocations as they occur in the circumference they are doubtless due to washing out of softer portions or some other well-understood freak of erosion.

Meteor Butte, then, seems to be merely the reverse of a solitary mesa which preserves itself by a hard roof against erosion. The Meteor Sink had a soft spot where its hat ought to have been.

FREDERICK S. DELLENBAUGH

NEW YORK, N. Y.

DIASTROPHISM AND DISCOURTESY

IN youthful days the writer regarded geologists as supermen, devotees of the most inclusive and inspiring of all the sciences. But membership in the guild, for lo, these many years, reveals that we are ordinary mortals, with the common frailties of human-kind.

In his address to Section E, American Association for the Advancement of Science, as reported in this journal of January 17, page 53, Dr. Frank Leverett stepped out of the path of his address to make an ungenerous and unjust personal reflection, as follows:

> In this connection attention is directed to an erroneous map prepared by a leading American glacialist, in which isobases of postglacial uplift are made to correspond to an estimated thickness of the ice-sheet in the region east of the Mississippi River, thus disregarding the results previously published of observations showing that there is no such correspondence. No progress can be made where office speculation is substituted for or given more weight than field studies.

A perusal of my description and discussion of the "erroneous map" (Bulletin of the Geological Society of America, 29, 1918, 201–205) will convince the reader that the implied discredit in "office speculation" is not justified. It would appear as if the speaker had merely looked at the map and neglected the accompanying explanation. Evidently he ignores it, and regards "previously published observations" as final and sacred.

The following excerpt from my paper (page 203) is only part of the tentative and suggestive matter which was, and is, wholly justified, as will be shown later.

> The map shows that the postglacial land uplift of northeastern America is fairly proportionate to the area and thickness of the latest ice-sheet, and it appears legitimate to suggest similar relation in the Mississippi

and Great Lakes region. The southward curve given to the lower-value isobases may be excessive, but they suggest an uplift for which evidence should be sought.

It should be understood, therefore, that the isobases as extended in the Mississippi Basin are intended to be only suggestive of the southerly limit of the Pleistocene land uplift, . . .[1]

The personal element may be dismissed, but the involved geologic problem merits brief discussion. The isobases of postglacial continental uplift in my map were based on careful field study, extended in both area and time; with included data from Canadian explorers. That the area of diastrophic movement, postglacial land uplift, closely coincided with the glaciated area, and that the amount of uplift, where clearly determined, was in close agreement with the supposed thickness of the ice-sheet was fact from observation and was not theory.

Because a tilting uplift is not registered in the glacial lake beaches in the southern ends of the Michigan and Erie basins, the assertion is made that there was an entire absence of uplift in the territory on south. But such assertion, with disdain for different opinion, is not proof. Lack of positive evidence is often inconclusive.

Along the border of the glaciated territory wherever water-planes existed to record land warping there was postglacial uplift to the extreme reach of the ice-sheet. This applies to all the area east of New Jersey.

With an exception to be noted below there were no extensive bodies of water over Illinois, Indiana and Ohio to register differential uplift. But with a thickness of several thousand feet of ice of the earlier ice invasions pressing down on that territory it seems highly improbable that it was not affected like lands under even thinner ice in the eastward areas.

Following the slow removal of the weight of ice, from south to northward, the rise of the land surface evidently by a wave-like uplift, progressing northward over the United States. The land rise about the periphery of the depressed area was earlier in time than northward toward the center of the ice load. Hence it should be expected that the southern ice-covered area in the Mississippi Basin would be upraised before the area of the Great Lakes. And such rise was either previous to or during the life of the glacial lakes Chicago, Maumee and Whittlesey. For that southern district the rise was "postglacial," but in what was yet "glacial time" for the northern lands.

[1] This map was republished in SCIENCE, Vol. 47, 1918, page 166. It carries one serious error. Newfoundland is represented as an area of uplift distinct from that of North America. The contributed information on which that mapping was based was later found erroneous.

However, we do have a bit of positive evidence of postglacial land warping in the southern area. In 1914 Professor George D. Hubbard described the tilted shorelines of an extinct lake in Alaska and Wayne counties, in north-central Ohio.[2]

This "Craighton Lake" was eighteen miles long, and the differential uplift of the shorelines was determined as about four feet per mile. This study has been adversely criticized because it was unexpected and contrary to theory.

In the same address (page 53), after assuming the absence of uplift south of the Michigan and Erie basins, although granting a thickness of thousands of feet of ice, Leverett makes the following statement:

> These studies and studies in other basins occupied by glacial lakes have shown that the uplift extends only a short distance beyond the Precambrian lands into the lands covered by Paleozoic formations. There appears to be a closer correspondence with the border of the Precambrian lands than with the amount of ice weighting. It appears that the ice weight was insufficient to cause such a depression in the stable areas covered with sedimentary Paleozoic formations as it was able to produce in the highly eroded Precambrian areas.

Every text-book of geology will show the main Precambrian area lying north of the Great Lakes and the Ontario-St. Lawrence valley. Southward, the Precambrian patches are the core of the Adirondacks, the Hudson Highlands and a portion of New England. But all the Great Lakes area and all the continent east of the Hudson Valley has participated in the postglacial uplift. From north of Lake Ontario to New York City and from north of the St. Lawrence to the coast of Nova Scotia is not "a short distance" into the sedimentary formations.

The central area of postglacial uplift is not in the midst of the great Precambrian mass, but lies near the south border of that mass, southeast of James Bay, between that bay and Quebec City.

This center of the dome-shaped land uplift is presumably the area of the greatest depression under the ice-weighting. And evidently the latter was in consequence of the greater thickness of the ice. The central area of the postglacial uplift quite certainly locates the center of the Quebec (Laboradorian) ice cap, its feeding ground or "alimentation area." And such location was not affected by the nature and age of the underlying rocks, but was determined by the snow supply, which in turn was dependent on altitude and the atmospheric circulation.

The postglacial land uplift everywhere, as far as it has been clearly measured, appears to have direct relation to the thickness and weight of the ice load.

[2] *Amer. Jour. Science*, 37, 1914, 444–450.

It has no apparent relation to the character of the underlying rocks, nor to the land relief or the gross topography. And why should it? The cause of the up and down (diastrophic) movement of the land surface can be only slightly due to the elastic compression and expansion of the rocks. It is regarded as chiefly due to yielding and rock-flowage in the deep-down zone of plasticity. And this is very far below the base of any sedimentary rock.

Another interesting fact is that the isobases, or lines of equal uplift, pay no apparent respect to the great topographic features, as the deep and wide embayment of the St. Lawrence and the masses of the Adirondacks and White Mountains. And, again, why should they? These great features were produced far back in Tertiary time, and isostatic equilibrium had been long established, for both the nature of the rocks and the surface relief, before the Glacial Period. The ice caps were freshly imposed loads, with independent effect.

H. L. FAIRCHILD

THE CLASSIFICATION OF PYTHIUM

THE writer has read with some interest a note in a recent number of SCIENCE by C. P. Sideris entitled "The Proper Taxonomic Classification of Certain Pythiacious Organisms,"[1] as he has been investigating for some years those members of *Pythium* which possess filamentous sporangia, placed by Butler[2] in the subgenus *Aphragmium,* and has had an opportunity to examine minutely most of the newer species and nearly all the older ones.

It should be borne in mind that the genus *Pythium* was founded in 1858 by Pringsheim[3] on what must be regarded as a form possessing entirely filamentous sporangia (the term "sporangium" will be used here in its older sense without entering into the grounds for the distinction of pro- or pre-sporangium used by some more recent writers). Two years later de Bary[4] published his description of *P. proliferum,* a form in which the sporangium consisted of a spherical portion and a more or less elongated beak, the former structure being delimited from the rest of the hypha by a cross wall.

With the describing of *P. debaryanum* by Hesse,[5] in 1874, and de Bary's subsequent work on this species, *Pythium* became of peculiar interest to the pathologist because of the destructiveness of this species to the seedlings of various plants of economic importance. Since then, the pathological literature has contained many references to various types of diseases ascribed to *P. debaryanum,* and through it the genus has been, one might say, widely advertised.

In recent years it has become increasingly apparent that, aside from the two sporangial types heretofore described, there is a third one. Briefly, this consists of a basal portion of more or less compacted, swollen, digitate elements, separated as a whole by cross walls from the concomitant hyphae, and a filamentous evacuation tube through which the protoplasm of the two portions is discharged into a vesicle in the usual manner. This type is represented by such forms as *P. complens* Fischer, *P. aphanidermatum* (Eds.) Fitz. and others.

Fischer[6] in 1892, divided the species of *Pythium* then known into three subgenera. In *Aphragmium* he placed those forms which possess filamentous sporangia not differing from the vegetative hyphae and not cut off from these structures by cross walls. In *Nematosporangium,* he placed those forms with filamentous sporangia which did not differ from the vegetative hyphae, but were separated from them by septa. The species possessing subspherical to spherical sporangia he put in the subgenus *Sphaerosporangium.*

In 1897, Schröter[7] raised *Nematosporangium* to generic rank with two subgenera, *Aphragmium* and *Eunematosporangium.* Butler[8] proposed to retain the two subgenera of Fischer, but merged *Nematosporangium* with *Aphragmium.*

To any one who has studied the non-sexual reproduction of any of these filamentous types, it is apparent that cross walls must be laid down somewhere in the mycelium which will limit the flow of protoplasm, otherwise the whole content of the mycelium would be discharged at one time into the vesicle. In the hundreds of examples of such reproductive activity observed by the writer among various species which possess entirely filamentous sporangia, delimiting cross walls have always been observed.

These preliminary considerations lead to Mr. Sideris's suggested treatment of the genus.

Aside from the fact that he does not separate the entirely filamentous sporangial forms, such as *P. dictyosporum* Racib., *P. afertile* Kanouse and Hum-

[1] C. P. Sideris, "The Proper Taxonomic Classification of Certain Pythiacious Organisms," SCIENCE, 71: 323–324, March 21, 1930.

[2] E. J. Butler, "An Account of the Genus Pythium and Some Chytridiaceae," Mem. Dept. Agr. India, Bot. Series 1: 5, 162 pp., illus., 1907.

[3] N. Pringsheim, "Beitrage zur Morphologie und Systematik der Algen II," *Pringsheim's Jahrb. für wiss. Botanik,* 1: 284–306, 1858.

[4] A. de Bary, "Einige neue Saprolegnieen," *Pringsheim's Jahrb. für wiss. Botanik,* 2: 169–192, 1860.

[5] R. Hesse, "Pythium debaryanum, ein endophytischer Schmarotzer, etc.," Inaugr. Diss. Halle, 1874.

[6] A. Fischer, "Phycomycetes," in Rabenhorst's "Kryptogamenflora von Deutschland, etc.," 4: 391–410, 1892.

[7] J. Schröter, "Fungi," in Engler and Prantl's, "Die natürlichen Pflanzenfamilien," I[1]: 104, 1897.

[8] E. J. Butler, *loc. cit.*

phrey, etc., from those herein termed "lobulate" types, as represented by *P. aphanidermatum, P. arrhenomanes* Drechsler, etc., he applies the name *Pythium,* not to those entirely filamentous forms on which the genus was founded, but to those with spherical sporangia. In the amended form, as proposed by Sideris, *Nematosporangium* groups together species having two different types of sporangia, *i.e.,* the entirely filamentous and the lobulate ones. *Nematosporangium,* in the sense of Fischer and Schröter, fits well those forms placed in *Aphragmium,* for, as has been pointed out, Aphragmium types are an impossibility. As the name *Pythium* already exists for these forms, and, in fact, was first applied to the very species placed by Schröter in his genus *Nematosporangium* (*P. monospermum*), the latter generic name is superfluous.

The situation might be somewhat clarified if the lobulate types were placed in the genus *Rheosporangium,* the first generic name proposed for these forms (Edson).[9] While some slight changes would have to be made in defining the genus, it seems better to adopt it than to continue, in a greatly amended condition, an untenable and confusing one. This action would leave Pythium in its original sense applying to those forms possessing entirely filamentous sporangia. As to the disposition of the subspherical and spherical sporangial forms, the writer does not feel so competent to express an opinion. Several courses, however, seem open: to raise the subgenus *Sphaerosporangium* to generic rank; to merge the group with *Phytophthora,* established by de Bary in 1876, or to revive one of the several generic names proposed in the past for various members of this group. If the genus *Pythium,* in the inclusive sense of Butler, is to be broken up into several genera, as seems entirely feasible, the name *Pythium* should apply only to those entirely filamentous sporangial types on which the genus was founded and not to those forms with spherical sporangia.

The salient features by which the three genera may be separated from each other are as follows.

Pythium: Zoosporangium identical with the filamentous vegetative hyphae, consisting of a hyphal segment delimited by cross walls; the content being discharged through a hyphal branch into a vesicle formed at the apex of this structure; not proliferating; the discharged protoplasm entirely delimited into zoospores within the vesicle.

Rheosporangium: Zoosporangium consisting of a subspherical portion or a series of more or less compacted, intercommunicating, lobulate elements cut off from the vegetative hyphae by septa, and a filamentous evacuation tube which arises from the basal portion and through which the content of the whole complex is discharged into the vesicle; not proliferating; the discharged protoplasm entirely delimited into zoospores within the vesicle.

Phytophthora or *Sphaerosporangium* n. gen.: Zoosporangium narrowly ovoid, spherical or citriform, clearly distinguishable from the vegetative hyphae from which it arises and is separated by a cross wall; renewed by proliferation or branching of various types; vesicle present or absent; zoospores sometimes clearly delimited within the sporangium and emerging fully formed.

F. K. SPARROW, JR.

DARTMOUTH COLLEGE

[9] H. A. Edson, "Rheosporangium Aphanidermatus, A New Genus and Species of Fungus Parasitic on Sugar Beets and Radishes," *Journ. Agr. Res.,* 4: 279–291, 1915.

TWO NEW DOMINICAN FORMATIONAL NAMES

THE MARINE PLIOCENE GATO FORMATION

THE late Señor Rodolfo Cambiarso, a resident of Santo Domingo City, presented me, in 1916, with a very fine fossil sea urchin that he had collected at Gato, Provincia del Seibo, on Rio Chavon, northeast of the port of La Romana, Dominican Republic. This town is shown upon the Santo Domingo-San Juan folio map of the American Geographical Society of New York. I gave the specimen to the U. S. National Museum, and Dr. R. T. Jackson identified it as *Clypeaster dalli* Twitchell,[1] and later described it in his "Fossil Echini of the West Indies,"[2] leaving the age indeterminate, Miocene or Pliocene.

This very handsome Dominican specimen is the only example of the species ever recorded except the type which Twitchell[3] described from near Fort Thompson, Caloosahatchee River, in the Caloosahatchee Pliocene of Florida.

In 1929, I referred my Gato beds to the marine Pliocene.[4] No other marine Pliocene has been differentiated in the Dominican Republic. I now propose for this interesting horizon, characterized by *Clypeaster dalli* Twitchell, the name Gato Formation.

The Gato formation of the Dominican Republic was laid down about the same time as the marine Pliocene beds described by Drs. Woodring and Brown[5]

[1] C. J. Maury, *Bull. American Paleontology,* No. 30, pp. 20–21, 1917.

[2] R. T. Jackson, Carnegie Inst. Washington, Publ. No. 306, p. 37, 1922.

[3] Clark and Twitchell, Monograph U. S. Geol. Surv., Vol. 54, p. 218, Pl. 99, Figs, 2a-b; Pl. 100, Figs, 8a-b, 1915.

[4] C. J. Maury, SCIENCE, p. 609, December, 1929.

[5] Woodring and Brown, "Geology of Haiti," pp. 241–242, 1924.

on Rivière Gauche, arrondissement of Jacmel, Republic of Haiti.

The Upper Miocene Caimito Formation

The lithological and faunal characters of the beds at Caimito, on Rio Cana, in the valley of Rio Yaque del Norte, Dominican Republic, were described by the writer[6] in 1917. The beds were found by the Maury Expedition, and the horizon was seen to be very intriguing, and an Upper Miocene age was suspected.

Later, from researches of the Vaughan Expedition, the Cerro de Sal formation, on the southern side of the Dominican Republic, in the Province of Barabona, near Las Salinas and Angostura, was described by Messrs. Condit and Ross[7] and referred to the Upper Miocene.

In 1929, I definitely referred the Caimito beds to the Upper Miocene,[8] and now propose for this horizon the name Caimito Formation.

Correlation of Upper Miocene Antillean Horizons

The following correlation of the Upper Miocene beds of the West Indies is now suggested. Those of Trinidad Island were discussed in detail by the writer in 1925.[9]

Upper Miocene: Younger or Caimitoan stage, Dominican Republic, north side, Caimito formation; south side, Cerro de Sal formation, apparently slightly younger than the Caimito. Trinidad, west central part of the island, Gomez Estate beds and Freeport to Todd's road outcrops. Tobago Island, Botanic Station beds. *Upper Miocene: Older or Springvalean stage,* Trinidad, Savanetta and Springvale beds near Couva, in the western part of the island; Pointe Noir beds on the eastern side of the island.

Climate of the Caimitoan Stage: Gypsum in the form of encrustations, plates and crystals is present in the Caimito formation. It is also abundant in the Cerro de Sal formation where it is followed by salt, which runs about 90 per cent. sodium chloride. This indicates aridity and excessive evaporation towards the close of the Upper Miocene. Conditions recall the gypsiferous strata of the Catahoula formation, regarded by the writer as equivalent to the Tampa formation of the Lower Miocene. The Cerro de Sal horizon is probably somewhat younger than the Caimito because the degree of evaporation exceeded that required for the precipitation of gypsum, and the beds of nearly pure salt were deposited.

Salt marsh and shallow lagoonary habitats are indicated for the faunas of the Caimitoan stage, concomitant with a rising coast-line and withdrawal of the sea off shore.

Carlotta J. Maury

Yonkers, N. Y.

REPORTS

INVESTIGATIONS IN PANAMA DURING THE SUMMER OF 1930[1]

Introduction

At the suggestion and on the invitation of Dr. Herbert C. Clark, director of the Gorgas Memorial Laboratory, Panama City, the writer undertook the investigation which is presented in this report. Facilities were extended so that Dr. William H. Martinez, a Tulane Medical School graduate, accompanied the writer, and later Mr. Alberto Prieto, a Tulane Medical School student resident in Panama, was enlisted as a technical assistant. In addition, non-professional personnel were secured locally in Panama City. The entire facilities of the Gorgas Memorial Laboratory and the connections which it enjoys in the Republic of Panama and the Canal Zone were placed at the writer's disposal for the work in view.

Objects of the Study

The investigations were undertaken with the following objects in view.

(1) To study representative samplings of the population of Panama, in order to determine by present refined methods of examination the incidence of *Endamoeba histolytica,* both in its active and carrier states, and to discover, if possible, the public health importance of this infection in Panama at the present time.

(2) To study concurrently the incidence and significance of the following infections: giardiiasis, ascariasis, hookworm infection, trichocephaliasis and strongyloidiasis.

(3) To obtain autopsy confirmation of as many of these cases as was possible.

(4) To treat selected cases of *Endamoeba histo-*

[6] C. J. Maury, *Bull. American Paleontology,* No. 30, pp. 25–26, 1917.

[7] "Geol. Reconnaissance Dominican Republic," pp. 214–215, 1921.

[8] Science, p. 609, December, 1929.

[9] *Bull. American Paleontology,* No. 42, 1925.

[1] Contribution from the Gorgas Memorial Laboratory, Panama, and from the Department of Tropical Medicine, Tulane University.

lytica with di-hydranol; to treat hookworm infection, where it seemed advisable, with tetrachlorethylene, and to treat cases of strongyloides infection with gentian violet.

(5) To determine whether *Endamoeba histolytica* is a natural infection in New World monkeys in the wild state or whether their infection, if present, is acquired from contamination with human habitations.

MATERIAL STUDIED

During the three months which were available for this investigation the following series of cases were examined.

(1) The Hospital Santo Tomas of the Republic of Panama: medical wards, 635; surgical wards, 166; maternity wards, 445; total, 1,246.

(2) Canal Zone Administrative Office employees and families (white population only), 143. Gorgas Hospital patients and staff (white population only), 153. (This group represents individuals enjoying the best sanitated condition of the Canal Zone).

(3) Four Chagres River villages, 542.

(4) Tiura River villages (Darien Province), 105.

These made a grand total of 2,089 cases. Of this number three or more fecal examinations were obtained on 1,340 individuals; two examinations on 326 individuals, and one examination only on 423. The total number of fecal examinations was in excess of 6,000, making an average examination quotient of approximately three per individual.

BRIEF RÉSUMÉ OF THE FINDINGS

(1) *Endamoeba histolytica.* (a) In the Santo Tomas Hospital, which represents the average cross section of the Panamanian population, the incidence of *Endamoeba histolytica* ranged from 12 to 18 per cent., the former being on medical wards and the latter on maternity services. (b) Canal Zone white administrative force employees, 2.81 per cent. (c) Gorgas Hospital patients and staff, 8.55 per cent. (d) Chagres River villages, 34 per cent. (e) Tiura River villages, 30 per cent. on one examination (children, 1 to 15 years, 38 per cent.; adults, 15 years or older, 18 per cent.). This last figure is based on one examination only, so that on a basis of three to six examinations the figure for children would range above 75 per cent. (f) Monkeys obtained in the wild state above the town of Boco de Cupe on the Tiura River, Darien Province, provided the following results: marmosets, negative for endamoebae; white-face monkeys, negative for endamoebae; black howling monkey, large numbers of active *E. histolytica, E. coli,* and *Endolimax nana.* In addition, autopsy examination of one red spider monkey, which had been in the animal house of the Gorgas Memorial Laboratory for several months, revealed a condition similar to that found in the black howling monkey. Likewise, a baby black howling monkey, which had been in the laboratory for less than one month, showed, on examination of passed feces, numerous cysts of *Endomaeba histolytica.*

Through the cordial cooperation of Dr. L. C. Prieto, of the maternity service of Santo Tomas Hospital, a considerable series of cases positive for *Endamoeba histolytica* received di-hydranol treatment. In every case the drug was effective in clearing up active or obscure symptoms involving the large bowel, and follow-up examinations showed that the dysentery amoebae had disappeared from the stools. A special paper will communicate these findings in detail.

Autopsies were obtained on five cases which had been previously diagnosed as harboring *Endamoeba histolytica.* Of this series two showed extensive deep chronic amoebic ulceration of the cecum, colon and rectum. The other three, which were carrier cases, showed no lesions which were detectable either by gross examination or by microscopic findings.

(2) *Giardia lamblia.* This infection ranged from 4½ to 9 per cent. in the Panamanian populations examined. It was more common in children than in adults.

(3) *Ascaris lumbricoides.* This worm was relatively common in the native populations, particularly in the country districts, but was fairly light as respects the number of worms in each case. As a filth infestation it was found to be more common in children than in adults.

(4) *Necator americanus.* This worm was also found to have a wide distribution but to be of relatively little clinical importance in the populations studied. It was not common among children under eleven years of age. Treatment with tetrachlorethylene on the maternity services of the Santo Tomas Hospital indicated the value of this drug in the treatment of such cases where toxic complications are to be avoided.

(5) *Trichocephalus trichiurus.* The infection with this worm ranged from seven tenths of one per cent. among the Canal Zone employees to 21 per cent. incidence in the Santo Tomas Hospital and river-town populations. Most of these cases consisted of light infestations only, but from 15 to 20 per cent. constituted conditions of clinical significance. One case in particular, which was observed in the maternity service of the Santo Tomas Hospital, called for special consideration. This woman was almost *in extremis* at the time when observation was first begun. Her stools were watery in consistency and teemed with Strongyloides larvae. Treatment with gentian violet *per os* was instituted and improvement noted

after ten days, with larvae reduced to a minimum. By the end of three weeks her condition was markedly improved and the stools were formed. In less than five weeks she had left the hospital, having gained several pounds and with evidence pointing toward complete recovery.

Thirty-two heavy strains of Strongyloides were cultured and examined daily over a period covering approximately two and a half months. Five of these were duplicate strains from the same individuals. Of the total number, twenty-three strains showed direct development only, four showed indirect development only and five consisted of combined types. Clinical importance is attached to those direct strains which metamorphose into the filariform larvae without a previous feeding period. Larvae of this filariform type are frequently passed in stools of individuals showing clinical symptoms. Evidence is accumulating to indicate that this is the type which is responsible for hyperinfection of individuals.

Rare Parasites Encountered

Balantidium coli. This ciliate protozoon was encountered in four cases during the investigations. These cases gave a history of probable contamination from porcine sources. Red spider monkeys in the animal house also had this same infestation.

Hepaticola hepatica. This infection is common in rats and mice in various parts of the world but there is only one human case on record, from autopsy of a British soldier in India. Nine cases of this rare human infestation were diagnosed from the Chagres River Basin.

Gongylonema pulchrum. One case of this rare human infestation was diagnosed on the basis of eggs from the Chagres River Basin.

Examination of Animals in the Jungle of Darien Province

Altogether 45 animals were examined during the trip up the Tiura River in Darien Province. The majority of these were monkeys. Filarial infections were found in two types, the marmoset and the white faced monkeys. In the poncho (*Hydrochoerus hydrochoerus*) amphistomate flukes were obtained from the abdominal cavity. This is possibly the same fluke which was recovered by Dr. Clark from the wild hog in the Coto region of Panama in February, 1929. Other helminth and protozoa parasites were obtained from the agoutis, the ponchos and the monkeys.

Conclusions

The material obtained from these investigations has indicated to the writer that the area studied offers extremely valuable opportunities for helminthological and protozoological work. The data obtained will serve as the basis for several important papers which will be published in the near future.

ERNEST CARROLL FAUST

TULANE UNIVERSITY

SCIENTIFIC APPARATUS AND LABORATORY METHODS

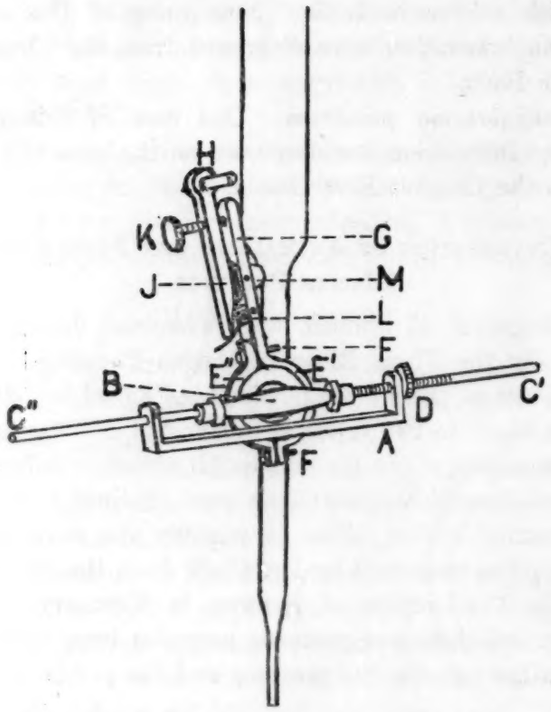
AN ADJUSTABLE DROP-CONTROL FOR BURETTES

For some time past, the writer has been performing micro-titrations to determine free and total acidities on one half to one cc of gastric juice. To insure an accuracy of better than one per cent., the burette employed was graduated so that one could estimate volumes to 0.002 cc. Since end-points were determined by comparison with colorimetric standards of definite pH, it was necessary to use drops of this order of magnitude in approaching the end-point. In order to obtain such drops, the burette tip was replaced by a Leuer adapter and a hypodermic needle (chromium plated or non-rusting steel) of such size as to give a freely falling drop of no more than 0.01 cc. Then, by opening the stopcock very slightly a small fraction of a drop was released, which could be taken off quantitatively with a stirring rod. However, the time and energy consumed by this procedure were so great as to cause an appreciable reduction in the efficiency of the worker engaged in these titrations. Therefore, a mechanical drop-control became imperative.

Such a device has been described by Müller[1] for use in potentiometric titrations where drops of uniform size are required. On attempting to employ his contrivance, two difficulties were encountered. Occasionally, the stopcock opened accidentally, due to a falling of the heavier end of the drop-control handle attached to the stopcock plug. Also, since it was necessary to add fractions of a freely falling drop, it was important that the rate of drop formation be readily controllable—which was not true in this case. Consequently, the requisite changes in the principle of Müller's device were made, and as the resulting instrument has proved very satisfactory for more than a year, it is described herewith.

It consists of two parts as indicated in the figure. The stationary part (EGJH), which carries the stop

[1] Erich Müller, "Elektrometrische (Potentiometrische) Massanalyse," 4th ed., 1926, p. 67.



(H), is attached to the sleeve of the stopcock; the movable handle (A), which strikes against the stop, is attached to the glass plug. Both parts are made of brass. To insure rigidity of the handle, the rod (C′) can be screwed tight against the handle of the plug (B) and can be clamped there by means of the nut (D).

The stationary part of the drop-control is clamped to the wider end of the glass sleeve by means of the semi-circular bands (E′ and E″), which are fastened together by two screws (F). One of these bands (E′) is an extension of the body (G); the other band is separate from it. The rod (H) serves as a stop to the rotating rod (C′ or C″) when it is desired to release a drop of reagent. This stop is set perpendicularly into the arm (J) which in turn is supported by (G). Support is effected by means of the projection (M) which passes through a hole in (G) and is held there by a pin, thus permitting a slight rotation of the arm (J) about the pin as fulcrum. The rotation is controlled by the screw (K) which passes through (J) and rests against (G). Movements of the arm, imparted by the screw, are opposed by the spring (L), which is held in a second hole in (G) by means of a pin. The free end of the spring is extended in such a way as to press against the lower end of (J).

The chief precaution to be taken in attaching the movable part to the plug is to have the glass handle fairly well centered with respect to (A). In attaching the stationary part, the body (G) is set at such an angle to the vertical that when the rod (C″) touches the stop (H), drops will flow from the burette tip at a slow but steady rate. Then, by means of the adjusting screw (K), the stop can be set at such a position as to permit the formation of freely falling drops at either a rapid rate or else so slowly as to enable the operator to remove small fractions of a drop at his convenience. On the other hand, it is always possible to get a steady flow of reagent from the burette by reversing the direction of rotation of (A) so that (C′–C″) is vertical. A strip of heavy rubber sheeting interposed between the semi-circular bands and the stopcock will prevent the latter from cracking.

The writer wishes to express his obligations to the departmental machinist, Willy Appledorn, for his assistance in constructing this instrument.

FRANKLIN HOLLANDER
DEPARTMENT OF PHYSIOLOGY,
N. Y. HOMEOPATHIC MEDICAL COLLEGE

SPECIAL ARTICLES

THE RELATIONSHIP BETWEEN ELECTRICAL DIFFERENCES OF POTENTIAL IN THE SKIN AND NORMAL BASAL METABOLISM

IN 1919 and 1920 Waller published the results of his researches on emotive phenomena and their correlation with variation of conductance of the palm of the hand. In general, Waller's experimental observations substantiated his view that there is "a close association between nutrition and what we may call emotion" and that there is reason for placing the "special class of emotive effects in the general category of trophic phenomena."[1] The experiments of Waller evidenced, therefore, a correlation between the electric resistance (or conductance) of the skin and nutritional changes and indicated that the emotive effect varied considerably in magnitude depending on whether the subject was rested or fatigued. No correlation between electrical conductance and general metabolic activity was attempted.

Waller was of the opinion that emotive effect is to be regarded as due to diminished resistance (increased conductance) rather than to increased electromotive force. However, Lund[2] and his colleagues

[1] A. D. Waller, "Concerning Emotive Phenomena. Part II. Periodic Variations of Conductance of the Palm of the Human Hand," *Proc. Roy. Soc. London*, Series B, 91: 17–31, 1920.

[2] E. J. Lund and W. A. Kenyon, "Relation between Continuous Bioelectric Currents and Cell Respiration. I.

very pertinently pointed out in the first of their series of articles on bioelectric currents and cell respiration that "methods which determine only the electromotive force and the changes in electromotive force are to be preferred when clearness of thought and interpretation and a greater nicety of distinction are to be attempted."

Lund concluded from his investigations on bioelectric currents and cell respiration that "the experiments definitely prove that the electric polarity of the cell is quantitatively correlated to the respiratory exchange of the cell and that electric currents accompany cell oxidation."[3] Lund[4] also advanced the theory that bioelectric currents produced by cells and tissues are the result of oxidation-reduction potentials developed by the respiratory mechanism of the cell, and that stimulation changes the electrical potential and therefore the electric polarity of the cell because it temporarily accelerates the reaction reductant → oxidant, thereby changing the ratio of oxidant to reductant. Data indicate the applicability of the familiar equation for oxidation-reduction potential

$$E = E_o - \frac{RT}{nF} \log \frac{[ox]}{[red]}$$

to the various phenomena studied.

Child[5] has written recently an excellent résumé of investigations on physiological gradients. Electrical potential differences are present along physiological axes (at least in many of the simpler organisms). In his consideration of differential susceptibility he pointed out that, in spite of criticisms, the data at hand show or indicate that differences in susceptibility to certain reagents are paralleled generally by quantitative metabolic differences. In general, "the protoplasmic factors immediately concerned in originating potential differences may be various, but that potential differences must be associated with the graded quantitative metabolic and other differences characteristic of the axial gradients appears to be beyond question."[6]

Sheard and Johnson[7] have shown that, in response to various qualities and quantities of radiant energy, there is a definite correlation between the electrical potential differences developed across areas situated near the base and tip ends respectively of leaves intact with plants.

These experimental results and theoretical considerations as well as others not cited led us to investigations concerning the possibility of a relationship between differences of electrical potential in the skin and the basal metabolisms of persons who were said to be normal clinically.

We have used, with slight modifications, the nonpolarizable type of electrodes developed by Alvarez, Freelander and Clark.[8] Potential differences were measured by standard potentiometric methods or by a portable combination of apparatus consisting of a millivolter and a galvanometer. One of the nonpolarizable electrodes was placed on the skin of the forearm just above the articulation of the ulna and radius at the wrist; the second electrode was placed at an arbitrarily chosen but fixed distance of 12 cm. The distance of separation of the electrodes need not be very exact since a change of 1 cm in the position of one of the electrodes gives a negligible change in the potential difference between them. Furthermore, the difference of potential, due essentially to electromotive forces developed between two given areas on the skin at a given distance apart, is the same after any difference of potential between the electrodes *per se* have been taken into consideration. Such observations militate against the view that we are dealing with contact differences of potential.

We agree with the conclusions of Alvarez, Freelander and Clark that cleansing of the skin (as with water or alcohol) has little if any effect on the values of the potential differences obtained. It has been suggested that the presence or absence of perspiration would have an effect on the values of the potentials. In order to minimize such an effect, if present, we have applied the electrodes to the outer side of the forearm where the skin contains relatively few sweat glands. Waller believed that the emotive phenomena which he observed must be explained on a more comprehensive basis than that of sudomotor variation. It appears, therefore, that the condition of the surface of the skin where contacts are made with the electrodes is not of importance if the areas of contact are relatively small.

Readings on the difference of potential were made

Electric Correlation Potentials in Growing Root Tips," *Jour. Expt. Zool.*, 48: 333–357, 1927.

[3] E. J. Lund, "Relation between Continuous Bioelectric Currents and Cell Respiration. V. The Quantitative Relation between E_p and Cell Oxidation as Shown by the Effects of Cyanide and Oxygen," *Jour. Expt. Zool.*, 51: 327–337, 1928.

[4] E. J. Lund, "Relation between Continuous Bioelectric Currents and Cell Respiration. II. A Theory of Continuous Bioelectric Currents and Electric Polarity of Cells," *Jour. Expt. Zool.*, 51: 265–290, 1928.

[5] C. M. Child, "The Physiological Gradients," *Protoplasma*, 5: 447–476, 1929.

[6] C. M. Child and L. H. Hyman, "Studies on the Axial Gradient in *Corymorpho palma*. I. Respiratory, Electric and Reconstitutional Gradients," *Biologia Generalis*, 2: 355–374, 1926.

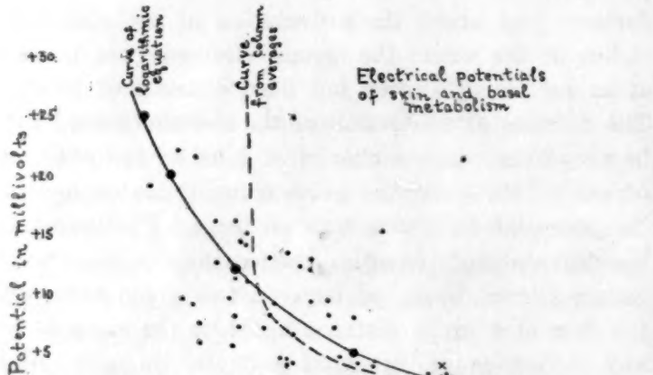
[7] Charles Sheard and A. Frances Johnson, "Potentiometric and Spectrophotometric Changes in Plants Produced by Infra-red and Ultra-violet Irradiation," *Proc. Soc. Expt. Biol. and Med.*, 26: 618–621, 1929.

[8] W. C. Alvarez, B. L. Freelander and L. B. Clark, "An Electrode for the Measurements of Skin Potentials," *Jour. Lab. and Clin. Med.*, 11: 83–85, 1925–26.

at various times during the period of rest of twenty minutes preceding the tests for the basal metabolism or again, in one group of subjects, during the metabolic tests. Basal metabolic rates were determined in certain groups with the Benedict-Roth closed circuit recording type of apparatus (measuring oxygen consumption) and in other groups by the open circuit gasometer followed by analyses of the expired air by the Haldane method. The Du Bois standards were used in our calculations.

The difference of potential between the electrodes applied to the skin ordinarily reached a fairly constant value after about ten minutes; if it did not, an average or a modal value was used. The true difference of potential then was obtained by subtracting the potential difference of the electrodes *per se* from the constant, average or modal value of the potential.

In Fig. 1 there are plotted the values of the potential differences as ordinates with the corresponding basal metabolisms, expressed in per cent. above or below normal, as abscissae in a group of persons presumably normal. The graph indicates a general tendency to grouping with some marked deviations. Treating the data statistically, a curve of the form



$$y = b\, e^{cx}$$

fits the data very well. With the insertion of the proper constants this equation becomes

$$x = \frac{\log y - \log 0.005}{-0.0396}$$

where x is the basal metabolism and y is the difference of electrical potential of the skin. The data of Fig. 1 show that higher basal metabolic rates are accompanied by lower differences of potential and *vice versa*.

Recently we have used the formula given in the preceding paragraph in an attempt to predict the basal metabolism. In a group of clinical subjects (some of whom had thyrogenous dysfunction) the predicted and observed values agreed within a range of ± 4 points in the metabolic rate expressed in percentage above or below normal in 62 per cent. of the individuals examined. The predicted basal metabolic rate agreed with the rate determined by test in nearly all cases (six exceptions) falling within the range of +13 per cent. to −10 per cent. Practically all the observed values which were not in agreement with predicted values fell either above +13 per cent. or below −10 per cent. The data as a whole, therefore, are divisible into three portions: (1) those showing a correlation between electrical differences of potential in the skin and basal metabolic rates such as are exhibited by normal individuals; (2) those above the normals, and (3) those below the normals.

It is known that individuals who have hypofunctioning thyroid glands manifest retardation in the rates of basal metabolism and circulation of the blood, whereas those who have excessive functioning of these glands show an increase in basal metabolic rates and in the circulation of the blood. We have carried out some experiments in which the circulation of the blood in the lower portion of the arm has been retarded or cut off by pressure and have observed marked increases of the differences of potential as compared to the values obtained under conditions of circulation of the blood ordinarily existing. Such observations indicate that there may not be a correlation of normal basal metabolic rates and differences of electrical potential of the skin if there are marked impairments of physiological functions, such as retarded (or accelerated) circulation, in the region in which the readings are made on the differences of potential. Since high and low basal metabolic rates (that is, outside the range of normal basal metabolism, +10 per cent. to −10 per cent.) are associated, respectively, in general, with a retarded or increased rate of circulation of the blood, it is entirely possible that the correlation lies between differences of electrical potential of the regions of the skin under test and circulatory conditions of the blood rather than between such potential differences and the basal metabolic rates *per se*. In normal, healthy individuals, however, with normal circulation of blood and normal physiological functioning of the skin there is a definite correlation between basal metabolism and differences of electrical potential.

The results of investigations now in progress regarding the electrical gradients in the skin and basal metabolism in various pathologic conditions as well as data concerning the general relationships existing between metabolic rates, skin temperatures, blood pressures, differences of electrical potential and so

forth, under controlled environmental conditions, will be published elsewhere in medical literature.

CHARLOTTE PURDY
A. FRANCES JOHNSON
CHARLES SHEARD
DIVISION OF PHYSICS AND BIOPHYSICAL RESEARCH,
THE MAYO FOUNDATION

STUDIES IN CERTAIN FACTORS AFFECTING THE RESISTANCE OF PLANTS TO INSECT PESTS

As far as the writer is aware, only two scientific papers have been written on the general subject of resistance of plants to insect pests. The first of these was published by McColloch[1] in Kansas in 1924, and the second by Lees[2] in England in November, 1926. An interesting but popular account of the problem was given by Treherne in 1917.[3] It has also been ably discussed in Wardle and Buckle's "Principles of Insect Control"[4] and in certain other text-books. Lists of references to observations on the resistance of plants to insect pests[5] are to be found in the published work of Mumford, Wardle and Buckle, McColloch, and Lees.

Resistance to insect pests may be due either (1) to some external protective agency, such as thickened epidermis or cuticle, the development of hairs, etc.; (2) to some condition of the cell-sap which repels the insects, such as the presence of certain oils, alkalis, organic acids, etc., or (3) though not actually unattractive or repellent, the sap may be unsuited to the insects' food requirements. The word "resistance" is also used in another sense—to denote the plant's ability to survive or "pull through" severe insect attack. In this case, though the insects still attack the plant, the damage is not apparent. Vigorous, healthy plants are said to "outgrow" the checking influence of attacks more readily than weaker, ill-nourished plants.

The problem of resistance is so complicated that the use of a special nomenclature seems advisable. The writer has proposed[6] the term *epiphylaxis* for external protective agencies, and *endophylaxis* for the internal protection afforded by biochemical qualities that render the plant unattractive or repellent or unsuited to the food requirements of insects. The importance of the water-balance as a factor in the resistance of plants to insect pests has recently been discussed by Mumford and Hey (1930).[7]

Examples of epiphylaxis.—An example of epiphylaxis is to be found in the protection afforded by the thick skin in certain varieties of *Citrus*. In Florida, it is the thin-skinned varieties of *Citrus* that suffer most from the attacks of such plant-bugs as *Nezara viridula* Linn. The thin-skinned tangerines, *Citrus nobilis* var. *deliciosa* and Satsumas, *C. nobilis* var. *unshiu* are the first choice of these bugs; oranges, *C. sinensis*, come second; grape-fruit, *C. grandis*, is but rarely attacked.

Further examples of epiphylaxis are found in the leaves, stems and bracts of the American Upland cotton, *Gossypium hirsutum* L., which are covered with short hairs that protect the growing plant. Sea Island cotton, *G. barbadense* L., and Egyptian cotton, *G. peruvianum*, possess no such protective covering. In South Africa, American Upland cotton has been found capable of resisting the ravages of the Jassid, *Empoasca facialis* Jacobi, whereas Sea Island and Egyptian cottons were the first to be attacked. Lloyd Worrall[8] also found a great difference in the protective character of the various Upland varieties, some having only a few scattered hairs whilst others were hirsute. Even within a given variety, certain strains possessed more protective characters than the average plant typical of that variety. When individual plants were seen to remain resistant when neighboring plants were badly attacked, in every instance these individuals were found to be covered with dense hairs on leaf, stem and bracts. Hairy types of cotton are not totally immune from injury due to Jassid attack, but in large measure they resist the insect to such a degree that the plant is enabled to mature its bolls before it is seriously injured.

Other plants possess similar agencies capable of

[1] J. W. McColloch, "The Resistance of Plants to Insect Injury," *Biennial Report of Kansas State Hortic. Soc.*, Vol. 37: pp. 196–208, Topeka, Kansas, 1924.

[2] A. H. Lees, "Insect Attack and the Internal Condition of the Plant," *Ann. App. Biol.*, Vol. 13, No. 4, pp. 506–515, Cambridge Univ. Press, 1926.

[3] R. C. Treherne, "The Natural Immunity or Resistance of Plants to Insect Attack," *Agric. Gaz., Canada*, Vol. 4, No. 10, pp. 855–859, Ottawa, 1917.

[4] R. A. Wardle and P. Buckle, "The Principles of Insect Control," Chapter 1, "Host Resistance," pp. 1–16, Manchester, 1923.

[5] The breeding of plants to resist either disease or insect attack has not been discussed in this paper, but is of the utmost importance. See McColloch, Wardle and Buckle, *loc. cit.*, and Babcock and Clausen, "Genetics in Relation to Agriculture," pp. 444–463, New York, 1927.

[6] "Cotton Stainers and Certain Other Sap-feeding Insect Pests of the Cotton Plant. A Preliminary Inquiry into the Effect of Climatic and Soil Conditions upon the Incidence of these Pests." 8vo, 79 pp., 14 pp. refs., London, Empire Cotton Growing Corporation, January, 1926, *Rev. App. Entom.*, Vol. 14, Ser. A., pp. 461–462, London, 1926.

[7] E. P. Mumford and D. H. Hey, "The Water-Balance of Plants as a Factor in their Resistance to Insect Pests," *Nature*, Vol. 125, No. 3150, pp. 411–412, March, 1930.

[8] L. Worrall, "Jassid-resistant Cottons," *Jl. Union S. Africa Dept. Agric.*, Pretoria, Vol. 7, No. 3, pp. 225–228, September, 1923; Vol. 10, No. 6, pp., 487–491, June, 1925.

resisting the attacks of insect pests—for example, the thickness of the epidermis prevents some aphids, *Aphis maidis* Fitch, from attacking teosinte, *Euchloena mexicana*. Thick-skinned varieties of apple are not so readily attacked by the apple maggot, *Rhagoletis pomonella* (Walsh). This has also been shown to be true of the papaya fruit-fly, *Toxotrypana curvicauda* Gerst., and the melon-fly, *Bactrocera* (*Dacus*) *cucurbitae* (Coq.), on melons.

Examples of endophylaxis.—Although the external covering of the plant is all important as a protection against the invasion of pathogenic bacteria and fungi, which are often admitted by the punctures caused by insects, factors included under the term endophylaxis are probably more efficient in affording protection against sap-feeding insects.

During a recent visit to Honolulu, the writer had the opportunity of examining ten representative samples of wood which had been buried in the ground with a view to testing their relative resistance to termites, *Coptotermes formosanus* Shir.[9] There seemed to be little doubt but that the hard woods were attacked as freely as the soft woods, and the examination suggested that it was chemical, rather than physical, factors which made certain woods resistant.

A high ratio of potash to phosphoric acid in the leaves of the tea plant, *Thea sinensis,* is said to increase their resistance to the attacks of the tea mosquito, *Helopeltis theivora* Waterh.[10] It has been found possible to make cabbage plants stronger and more resistant to the attacks of cabbage fly, *Phorbia brassicae* (Bouché) by the addition of sodium nitrate or ammonium sulphate to the surrounding soil. At the Kansas State Agricultural College, increased resistance of wheat to the Hessian fly, *Phytophaga destructor* (Say.), has been secured by the addition of sodium nitrate to the cultural solutions. Russian investigators have found that a high silica content in the cells of *Lithospermum arvense* offers some protection against aphids.

Comes[11] considers that, *ceteris paribus,* the receptivity to parasites in a given organ increases with the quantity of reducing sugars, and that resistance increases with the organic acids. As the quantity of reducing sugars, so much sought after by insects, increases in vegetable tissues, there is, according to Comes, a corresponding decrease in the organic acids. Acidity, he considers, is the plant's defense against its enemies.

Further researches may also show some connection between the resistance of plants to insect pests and the physico-chemical properties of their cell-sap. Studies should be made of the hydrogen-ion concentration, water-content, osmotic pressure and electrical conductivity of the cell-sap and their possible correlation to the resistance or susceptibility of plants to the attacks of sap-feeding insect pests.

Carter[12] has made an important advance in his work on the relation between the osmotic pressure of the plant sap and the relative susceptibility of plants to attack by his discovery that the sugar-beet leafhopper, *Eutettix tenellus* (Bak.), avoids extremely high sap concentrations if more suitable food is available. In a small area where two growth-forms of the same species were present, the higher concentration forms were found to be abandoned for those having a lower centration. Similar studies have recently been made on the sap of sugar beets,[13] but the relation of these to Carter's interesting researches has not yet been determined. The plants studied by Carter were *Atriplex rosea* and *Salsola* sp. Investigations have also been made into the physiological basis of the varying susceptibility of sugar-canes to the froghopper, *Tomaspis saccharina* Dist., in Trinidad.[14,15]

Other examples which might possibly be included under the term endophylaxis occur in the copious secretion of resin which floods the larval galleries, drowning the young larvae, when sal trees, *Shorea robusta,* are attacked by the borer, *Hoplocerambyx spinicornis* Neum., and in the exudation of a milky juice by the delicate latex hairs of lettuce plants at Singalong. This latex fastens the insect to the plant.

EDWARD PHILPOTT MUMFORD

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[9] D. T. Fullaway, "Termites or White Ants in Hawaii," *Hawaiian Forester and Agriculturalist,* Vol. 23, No. 3, pp. 68–88, Honolulu, 1926.

[10] E. A. Andrews, "Factors Affecting the Control of the Tea Mosquito Bug, *Helopeltis theivora* Waterh.," Ind. Tea Assoc., London, 1923.

[11] O. Comes, "La Profilaxia en Patoligía Vegetál," Reale Instituto d'Incorraggiamento di Napoli, 173 pp., Naples, 1916. *Bol. Agric. Técnica y Económica,* Vol. ix, No. 102, pp. 508–514, Madrid, June, 1917. Abstract in *Rev. App. Entom.,* Ser. A, Vol. 6, pp. 55–56, 1918.

[12] W. C. Carter, "Population of *Eutettix tenella* (Bak.) and the Osmotic Concentration of its Host Plants," *Ecology,* Vol. 8, No. 3, pp. 350–362, Brooklyn, N. Y., July, 1927.

[13] E. P. Mumford, "On the Curly-top Disease of the Sugar Beet; a Biochemical and Histological Study. Summary of Results," *Ann. App. Biol.,* Vol. 17, No. 1, pp. 28–35, 6 refs., 2 plts., Cambridge, February, 1930.

[14] E. P. Mumford, "Notes on the Froghopper Blight of Sugar Cane in Trinidad," *Bull. Ent. Res.,* Vol. 17, Pt. 2, pp. 139–150, 47 refs., London, October, 1926; (abridged as) "The Froghopper Blight," *West. Ind. Comm. Circ.,* Vol. 41, Nos. 714–715, pp. 49–50 and 73–74, 7 refs., London, 1926. *Rev. App. Entom.,* Vol. 14, Ser. A, Pt. 12, p. 619, London, 1926. "The Sugar-cane Froghopper," *West Ind. Comm. Circ.,* Vol. 40, No. 711, pp. 525–526, London, December, 1925.

[15] C. L. Withycombe, "Studies in the Aetiology of Sugar-cane Froghopper Blight in Trinidad, I. Introduction and General Survey," *Ann. App. Biol.,* Vol. 13, No. 1, pp. 64–108, Cambridge, February, 1926.